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# ARTICLES

# THE DYNAMICS OF NEGOTIATED PROTECTIONISM

VINOD K. AGGARWAL
*University of California
Berkeley*

ROBERT O. KEOHANE
DAVID B. YOFFIE
*Harvard University*

*Recent protectionism by the United States has principally taken the form of negotiated barriers to trade, such as voluntary export restraints. These barriers tend to evolve over time and to display three patterns, which we label* institutionalized, temporary, *and* sporadic *protectionism. Cartel theory and studies of the politics of protection suggest that the dynamics of negotiated protectionism will depend on three variables: the barriers to entry into an industry, the size of the domestic industry, and the exit barriers for domestic firms. Low barriers to entry will lead to institutionalized protectionism when the domestic industry is large and exit difficult; temporary protectionism results when the domestic industry is small and exit easy; and sporadic protectionism is likely when barriers to entry are high. Brief studies of U.S. protectionism in textiles and apparel, steel, footwear, televisions, and automobiles illustrate the value of this framework.*

Trade protectionism has often been thought of as a unilateral act by a sovereign state. Yet much protectionism during the 1970s and 1980s has been negotiated rather than unilaterally imposed. Voluntary export restraints (VERs) and similar arrangements have become the preferred means by which the United States has sought to cartelize industrial sectors threatened by imports. Such negotiated protectionism has been particularly evident in five industries in the United States during part or all of the last quarter century. In textiles and apparel, steel, footwear, television sets, and autos, the United States has negotiated trade restrictions with numerous foreign exporters. While the U.S. government has occasionally provided unilateral measures of protection for small industries, all U.S. administrations since World War II have been reluctant to countenance general schemes of protection for major industries. Yet, beginning with the Eisenhower administration's negotiations in textiles in 1956, virtually every U.S. president has actively sought VERs for economically and politically sensitive industrial sectors.

Most research on protectionism has sought to uncover why governments impose trade barriers and how those barriers affect the economy (Bauer, Pool, and Dexter 1972; Cline 1983; Destler 1980; Pastor 1980; Strange 1985). Some social scientists have focused on explaining the height and impact of trade barriers (Baldwin 1986; Lavergne 1983); others have looked at the ideological and political roots of protectionist policies (Goldstein

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1986a; Schattschneider 1935); and still others have looked at how restrictions such as VERs—quantitative measures negotiated with selective exporters—are different from other forms of protection, such as tariffs and global quotas (Bergsten 1975; Yoffie 1981). This article, by contrast, examines what happens to negotiated protectionism in the United States *after* agreements have been reached. Casual observers often assume that trade barriers persist automatically, that once a country begins down the slippery slope of protection, there is nothing to do but slide further. Yet this generalization is false for the United States over the last quarter century. Indeed, we have identified three quite different patterns of negotiated protectionism involving the United States during this period. Protectionism in some industries has been temporary, in others it has a long institutionalized history, and in still others it has been sporadic.

Under *temporary* protectionism, the United States negotiates agreements with one or several exporters, but it allows them to lapse after some initial period. The restrictiveness of the protection is generally not very high. Protectionist arrangements in the color television and footwear industries have reflected this temporary pattern. In the case of color televisions, the United States negotiated an Orderly Marketing Agreement (OMA) with Japan in 1977, which was followed by two OMAs with Korea and Taiwan in 1978.[1] The Japanese OMA was allowed to expire in 1980, while the Korean and Taiwanese agreements expired in 1982. Similarly, in footwear, the Carter administration negotiated OMAs with Taiwan and Korea in 1977, but in 1981 the Reagan administration allowed both OMAs to lapse. Despite substantial political controversy and import penetration rising above 75% of U.S. consumption, the footwear OMAs have not been renewed.

The second pattern is one of *institutionalized* protectionism, where protectionist agreements expand over time, becoming more complex and gradually encompassing larger numbers of exporting countries and more and more categories of products. Such restrictions persist across administrations. This pattern of increasing institutionalization has characterized protectionism in textiles and apparel since the mid-1950s. Restrictions in textiles and apparel began with a simple bilateral agreement between the United States and Japan that limited Japanese exports in a few categories of cotton textiles in 1957. Four years later, a multilateral agreement was negotiated that provided a framework for the U.S. and Europe to restrict the cotton textile and apparel exports of about 30 countries. By the 1980s, the industrial countries were protecting hundreds of categories of cotton, wool, and synthetic fiber textiles and apparel from all producing countries in the world (Aggarwal 1985; Yoffie 1983a).

The third pattern of protectionism follows a more *sporadic* course. In this pattern, initial agreements lapse, but protectionism is subsequently renegotiated. This nonlinear pattern can be observed most clearly in the steel industry. In 1968 the United States negotiated a tripartite agreement with the European Community and Japan in basic steel. Although the initial three-year agreement was extended through 1974, the Ford administration allowed protection to lapse. In 1977, however, the steel industry again managed to convince the Carter administration to reimpose trade barriers. These restraints have persisted in a variety of different forms through the mid-1980s.

The existence of different dynamic patterns of protectionism in different industries provides us with our major puzzle. How do we account for the varying patterns exhibited by negotiated U.S. protectionism? Because we are interested in the evolution of negotiated protectionism rather than in its origins, we begin at the point when protection has first been implemented on a sectoral basis. To account for patterns of change, we sketch an interpretive model that emphasizes the size of the industry and two additional variables not usually stressed in discussions of protectionism: (1) the height of economic barriers restricting entry of foreign producers into an industry and (2) the exit or adjustment strategies of the domestic firms. Having outlined an interpretive model of U.S. protectionism, we then discuss our five industry cases: textiles and apparel, steel, color televisions, footwear, and autos.[2]

## Explaining the Dynamics of Negotiated Protectionism

Negotiated protectionism is fundamentally different from tariffs and auctioned quotas (Bergsten 1975). Unilateral forms of protectionism attempt to shift the burden of adjustment from an importer to an exporter, while VERs constitute cross-national cartel agreements that seek to allocate market shares between exporters and importers, thereby sharing the benefits and burdens of protectionism. Furthermore, VERs restrict exports from specific countries in specific product categories and are generally negotiated on a country-by-country basis. Even though these arrangements are designed to raise prices for consumers (who are excluded from the game), they are supposed to allow a domestic industry to generate profits as well as to allow an exporter to reap scarcity rents. The more stable the cartel arrangement, the greater the likelihood that the domestic industry will become more profitable.

In order to understand cartel stability, it is essential to look at an industry's structure. While the effectiveness of oligopolistic coordination can be affected by many variables, such as the number of sellers in a market and product heterogeneity, cartels cannot function for very long without high barriers to entry (Chamberlin 1962; Itoh and Ono 1981; Kierzkowski 1984; Scherer 1980). If barriers to entry are low—for example, when there are few scale economies, low capital requirements, and little product differentiation—new firms (or countries) will quickly enter the market when they see abnormally high profits. In an industry such as apparel, where capital requirements are very low and technology is widely available, any effort to create a cartel would immediately draw new entrants and reduce the effectiveness of protection.[3] When barriers to entry are high, however—for example, because of high capital requirements (e.g., steel), unavailable technology (e.g., semiconductors), or high levels of differentiation and expensive distribution channels (e.g., autos)—it may take substantial investments and long periods of time for new entrants to take advantage of output restrictions.

The implication of this argument is that the height of economic entry barriers will to a great extent determine a cartel's success. Recall that VERs restrain particular exporters. High barriers to entry are conducive to the success of such an arrangement because under these conditions new exporters, unrestrained by the VER, will be unable to enter the U.S. market. Therefore, high barriers to entry should produce a relatively stable cartel. Conversely, low barriers to entry should vitiate the effectiveness of a VER because unrestrained foreign producers will immediately begin exporting to the U.S. and bid away any economic rents. Instead of reducing import pressure, VERs in industries with low barriers to entry may speed up the diffusion of production to new foreign manufacturers in countries whose exports have not been restricted by negotiated agreements.

The success or failure of VERs helps to determine their evolution over time, because success and failure set in motion different patterns of political action. To understand how this occurs, we need to make some assumptions about when firms demand trade barriers and under what conditions the United States government will be disposed towards supplying protection. The purpose of these assumptions is to allow us to predict how the initial success or failure of a VER is likely to affect the subsequent evolution of negotiated protectionism in a particular sector. Although we will not provide empirical support for these assumptions, we believe they are plausible and consistent with the existing literature on the politics of U.S. trade policy.

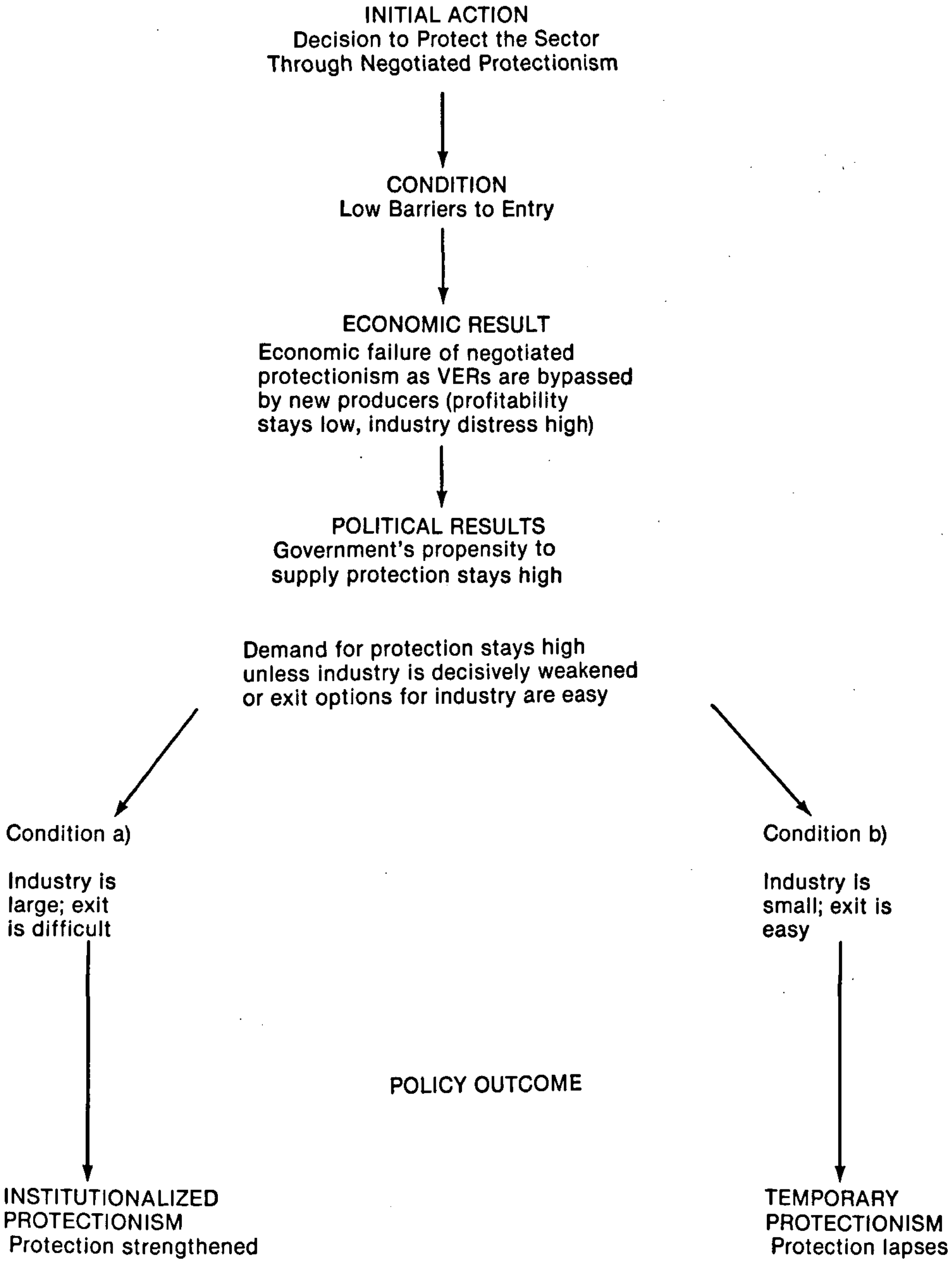
We start by assuming that the *demand* for protection generally intensifies when industries encounter economic distress due to growing import penetration (Baldwin 1986; Lavergne 1983). If firms are satisficers, they are most likely to demand trade barriers only when they confront declining production and profitability.[4] Furthermore, we assume that large industries with high levels of resources in votes, campaign funds, political organization, and so on will be able to pay more for protection than less well-endowed industries.[5] And finally, we assume that the more difficult it is for firms to leave the industry—that is, the higher the barriers to *exit*—the more the industry will be willing to invest resources in protection. As Hirschman has argued, the impossibility of exit stimulates voice (Hirschman 1971). Thus the demand for protection by an industry will tend to be a function of economic distress as indicated by profitability and employment trends and of the political resources available and the difficulty of exit.

On the *supply* side, we assume that U.S. governmental policy will not simply mirror industry demand: since the 1930s, many industries have pleaded unsuccessfully for protectionism (Lavergne 1983). This leads us to assert that the U.S. government is not merely a cipher, registering political pressures (Destler 1980). Instead, the willingness to supply protection will be a function of (1) the economic and foreign-policy costs of trade barriers and (2) the values of governmental officials who have their own policy preferences. With regard to costs, the U.S. government would be discouraged from imposing trade barriers if protection adversely affected politically influential domestic actors (e.g., by causing inflation for powerful downstream producers) or led to retaliation by trading partners. Indeed, if the U.S. employed tariffs and auctioned quotas instead of VERs, the inflationary consequences would be more transparent to domestic actors, and the U.S. would be obligated by the General Agreement on Tariffs and Trade (GATT) (Article 19) to provide compensation to its trading partners or allow retaliation.

Similarly, one can reasonably assume that governmental values, which in the case of the United States have been predominantly liberal in the postwar period, will also affect the supply of protection (Baldwin 1986; Goldstein 1986a, 1986b). For the purposes of our model, we assume that liberal values of the United States yield policy preferences that can be summarized in three injunctions:

1. Protection should be accorded only to firms and industries in distress, as manifested by low profitability and declines in employment that appear to result in large part from imports. The U.S. government offers protection to alleviate distress rather than to promote new or innovative industries that have the potential to be highly competitive.
2. Protection should be temporary, enabling industries to adjust, and should be quickly terminated for an industry that regains profitability.
3. If U.S. firms face "unfair" competition from abroad, it is legitimate for the government to provide sufficient protection to offset that unfair advantage.

Figure 1. Protectionist Patterns with Low Barriers to Entry


INITIAL ACTION
Decision to Protect the Sector
Through Negotiated Protectionism
CONDITION
Low Barriers to Entry
ECONOMIC RESULT
Economic failure of negotiated protectionism as VERs are bypassed by new producers (profitability stays low, industry distress high)
POLITICAL RESULTS
Government's propensity to supply protection stays high
Demand for protection stays high unless industry is decisively weakened or exit options for industry are easy
Condition a)
Industry is large; exit is difficult
Condition b)
Industry is small; exit is easy
POLICY OUTCOME
INSTITUTIONALIZED PROTECTIONISM
Protection strengthened
TEMPORARY PROTECTIONISM
Protection lapses

Using these assumptions about the supply of and demand for protection, we are now in a position to sketch our dynamic model. The logic underlying the model is relatively simple and straightforward. If protectionism fails to raise profits for a domestic industry, then, ceterus paribus, firms will increase the intensity of their demand for future trade barriers and the U.S. government will generally be sympathetic towards supplying additional protection.[6] Because we assume that industries work harder for protection when profits are low and that the U.S. government will provide temporary protection for firms in distress, trade barriers that are ineffective are likely to be expanded. On the other hand, if trade barriers improve an industry's economic health, then firms in that industry will reduce the intensity of their lobbying efforts, and the U.S. government will be less receptive to those pleas. Thus, for producers, there is irony behind the success of protection: the more profits domestic firms generate as a result of trade barriers, the more difficult it becomes to justify those restrictions. Today's success in lobbying for protectionist arrangements contributes to tomorrow's failure.

Because the critical issue is the success or failure of negotiated protection, we distinguish at the beginning between industries with high and low barriers to entry. The flowchart in Figure 1 illustrates what happens when barriers to entry are low. Negotiated protectionism will fail to restrict imports because the VERs will be bypassed by new producers. The profitability of domestic firms will stay low, and in view of the values we have ascribed to U.S. officials, the government's propensity to supply protection will remain high. The industry's desire for protection will also remain strong and may even increase. Frustration over continued low profits could lead to a more effective industry organization and an even higher level of protectionist pressure.

If the industry is large and faces high barriers to exit, our model predicts increasing protection: lacking exit possibilities, the industry will intensify its demands by exercising voice, and the government will supply further protection due to continued industry distress. Over time, the government will have to extend the VERs to more and more countries to increase the effectiveness of the protection. As larger numbers of producers become covered by the measures, such protectionism is likely to become institutionalized and may even lead to a system of global quotas, as indicated by outcome *a* in Figure 1.

A strengthening of protectionist pressure, however, is not the only possible result of economically unsuccessful protection. When an industry's *barriers to exit* are also low (Caves and Porter 1976), that industry may contract as firms leave the industry, causing it to lose politically potent resources.[7] Moreover, firms that anticipate continued competitive pressure from imports may shift from domestic manufacturing to importing, further reducing the demand for protection. If such an industry started off politically and economically weak, the exiting of firms may shrink the industry to such an extent that the remaining firms will no longer have sufficient resources to lobby effectively for added protectionism. Hence, our model predicts that a strikingly different pattern of protectionism will emerge when barriers to entry and exit are both low and the industry is small. Under these conditions, protectionist arrangements will be temporary as firms leave the industry and fewer resources are available for seeking protection (see outcome *b* of Figure 1).

Two other logical possibilities excluded from the chart for the sake of clarity, should also be mentioned. First, if an industry faces low barriers to entry and exit and also happens to be large, we would predict a pattern of institutionalized protection that would be unstable in the long run. As long as the costs of lobbying remain lower than the costs of exit, the demand for protection will remain high. Over time, however, this calculus may change. With VERs rather than global quotas, new entrants will continue penetrating the domestic market, industry distress will continue to be high, and domestic firms will increasingly exit. As a result, the industry will become weaker politically. The lobbying cost of protection for the remaining firms will increase, and exit will become even more attractive. In the long run, the industry will lose its critical mass, and the demand for protection will fall off.

A second possibility is that the barriers to *exit* might be high, and the industry small. Because barriers to entry are low, as in all the cases depicted in Figure 1, new entrants will continue to penetrate the domestic market. The ineffectiveness of protection will lead some firms to disappear or to exit despite the difficulties. This will weaken the industry, making it less and less likely to succeed in securing protection. Therefore, we expect protectionism under these conditions to be temporary, although its duration will be more extended than in the pure case of a small industry with low exit barriers.

In Figure 2, we display our expected sequence of events when barriers to entry are high. Because unrestricted foreign firms cannot immediately enter the U.S. market, protection will be economically successful, resulting in a slowing down of import penetration and higher profitability for domestic firms. In view of the values we have ascribed to the U.S. government, such success reduces the government's inclination to continue providing protection. At the same time, the demand for protection from the industry will also fall as its level of distress eases. In short, we expect protection for industries with high barriers to entry to be temporary in the initial round.

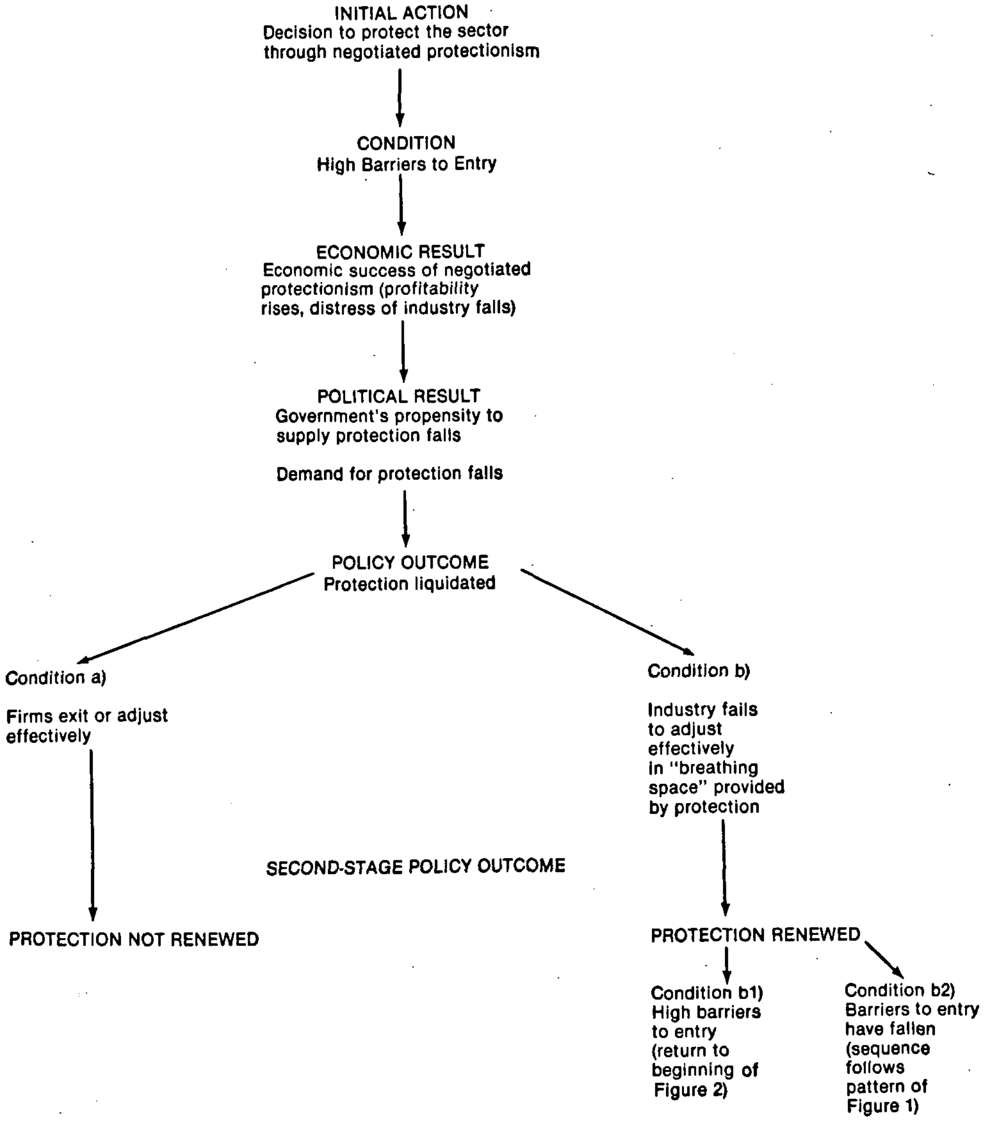
Suspension of protectionism will then lead to another round of import penetration, which under certain conditions may lead to a renewal of protectionism. Once again, our model regards the barriers to exit as an important factor. If firms can easily exit from domestic production, either by ceasing operations, moving production offshore, or becoming importers, the industry's demand for trade barriers will fall and protectionism will not be renewed (condition *a*).[8]

In industries with high barriers to exit that do not adjust during the period of protection, rising import penetration will lead to renewed industry distress (condition *b*) and renewed protectionism. Whether this sporadic protectionism continues depends on whether the barriers to entry fall over time. If the barriers remain high, the pattern of Figure 2 will repeat itself (condition *b*1)—the VERs will be effective, profitability will be restored, and the protectionism will lapse. However, if barriers to entry fall in the meantime—due, for example, to technological change or increased sophistication in developing countries—the subsequent sequence of events will follow the course identified in Figure 1.

This long-run indeterminacy is a result of the fact that firms exercise choice. Our model provides us only with a first-cut structural explanation for different patterns of protection. We also have to take into account the survival strategies adopted by firms in the protected industries. Although the modal result of unsuccessful protection is renewed protectionist pressure, such pressure will fall if a small industry shrinks substantially as a result of cuts in employment and profitability, or if domestic firms become importers. Conversely, successful protection can lead not only to its own liquidation but to a renewal of protection in the future, depending on the adjustment and exit strategies followed by firms.

This interpretive model of the evolution of negotiated protectionism is only valuable if it helps us to understand the complex patterns that we observe in industries in which the United States has used VERs and similar restraints to protect domestic producers. We therefore turn to our empirical material. Does our model illuminate patterns of negotiated protectionism?

Figure 2. Protectionist Patterns with High Barriers to Entry


INITIAL ACTION
Decision to protect the sector through negotiated protectionism
CONDITION
High Barriers to Entry
ECONOMIC RESULT
Economic success of negotiated protectionism (profitability rises, distress of industry falls)
POLITICAL RESULT
Government's propensity to supply protection falls
Demand for protection falls
POLICY OUTCOME
Protection liquidated
Condition a)
Firms exit or adjust effectively
Condition b)
Industry fails to adjust effectively in "breathing space" provided by protection
SECOND-STAGE POLICY OUTCOME
PROTECTION NOT RENEWED
PROTECTION RENEWED
Condition b1) High barriers to entry (return to beginning of Figure 2)
Condition b2) Barriers to entry have fallen (sequence follows pattern of Figure 1)

## Illustrating the Model: Five Cases of Negotiated Protectionism

Our model of the dynamics of negotiated protectionism, which focuses on economic barriers to entry, size of the industry, and barriers to exit, should help us to understand the varying patterns of U.S. protection since World War II. To illustrate this model, we will briefly analyze the protectionist patterns in five major industries: textiles and apparel, color televisions, steel, footwear, and automobiles. A summary of these cases is provided in Table 1. Our analysis of the first three cases is interpretive because the model was largely derived from our knowledge of protectionism in these industries. The latter two cases, however, provide us with a partial test of the model's explanatory power. The Reagan administration's decisions to deny further protection to automobiles in the spring of 1985 and footwear the following fall, proved to be remarkably consistent with our expectations.

### Institutionalized Protectionism: Textiles and Apparel

The textile and apparel industries, though strikingly different in their industrial characteristics, have followed a joint strategy in securing protection from imports since the late 1950s (Aggarwal 1983). The coalition strategy started to pay off in 1957, when the U.S. government reached an agreement with Japan that limited textiles and apparel exports for a period of five years. The Eisenhower administration opted for a VER largely because the domestic and foreign policy costs of any alternative in the mid-1950s seemed prohibitive. Tariffs or quotas on such a large industry would have been inconsistent with GATT and contrary to U.S. concerns about the Cold War and U.S. efforts to liberalize international trade. Yet, after the U.S. negotiated this VER with one country, few would have guessed that protectionism in this sector would evolve into the most complex, institutionalized system of restrictions in the postwar period.

Our model of protectionism begins to analyze this problem by looking at the size and characteristics of these industries. With more than two million workers in the mid-1950s, textiles and apparel combined were the largest employers in the manufacturing sector in the United States. Although textiles was more capital-intensive than apparel, both industries could be classified as labor-intensive in the 1950s with relatively low barriers to entry. Capital requirements were low compared to industries like steel; and technology, raw materials, and equipment were readily available. Industry profitability was also low: textiles and apparel mills earned a meager 1.4% and 4.5% return on equity (ROE), respectively, in the mid-1950s, compared to a 9.9% return for all manufacturing (*Congressional Record* 1966, 20985).

Under these conditions, our model would predict that a single bilateral agreement between the United States and Japan would quickly attract new entrants. With low barriers to entry, non-Japanese manufacturers would seek to capture any scarcity rents that might be generated by the proposed cartel, making it difficult to protect the U.S. market effectively. Imports should increase, the profitability of U.S. firms should remain low, and the demand for protection should rise. In fact, the 1957 VER failed to protect U.S. producers (Aggarwal 1985; Yoffie 1983a). Asian exports of cotton manufactures bound for the U.S. boomed. Hong Kong alone increased its shipments from under $1 million in 1956 to over $60 million by 1960 (Hunsberger 1964). The combination of rising imports and profits well below

**Table 1. Summary of Evidence**

| Industry | Protectionist Policies | Import Penetration[a] (%) | Pattern |
|---|---|---|---|
| Textiles & apparel | 1956 (VER, Japan 1957) | 2[b] | Institutionalized |
| | 1960 (STA/LTA, 1961-62) | 6[b] | |
| | 1970 (VERs 1971) | 9 | |
| | 1973 (MFA 1974) | 9 | |
| | 1976 (MFA 2, 1977) | 11 | |
| | 1980 (MFA 3, 1981) | 12 | |
| | 1985 (MFA 4, 1986) | 23 | |
| Footwear | 1976 (OMA, Taiwan & Korea 1977) | 48 | Temporary |
| | 1981 OMAs dropped | 50 | |
| | 1984 Protection rejected | 70 | |
| | 1985 Protection rejected | 76 | |
| Televisions | 1975 | 18 | |
| | 1976 (OMA, Japan 1977) | 33 | |
| | 1978 (OMA, Korea & Taiwan 1979) | 26 | |
| | 1980 OMA dropped (Japan) | 12[c] | |
| | 1982 OMAs dropped (Korea & Taiwan) | 19[c] | |
| Steel | 1968 (VER, Japan & EEC 1969) | 17 | Sporadic |
| | 1970 | 14 | |
| | 1971 (VER, Japan & EEC 1972) | 18 | |
| | 1974 VERs dropped | 13 | |
| | 1977 (TPM in 1978) | 18 | |
| | 1981 (VERs, Japan & EEC 1982) | 19 | |
| | 1984 (VERs 1985) | 26 | |
| Autos | 1980 (VER, Japan 1981) | 27 | |
| | 1985 VER dropped | 32 | |

[a] All import penetration based on quantity (weight for textiles & apparel).

[b] Import penetration for cotton products only.

[c] The drop in import penetration masks the movement offshore of U.S. firms and the direct investment in assembly by foreign producers. Total value-added of TVs manufactured in the U.S. was only 45%-47%.

[d] Annualized rates based on 11 months.

[e] To establish levels of import penetration prior to the conclusion of protectionist agreements, we have gathered data for the years immediately preceding the dates of such agreements. Parentheses are used to indicate that the years for which data are recorded and the years in which agreements were concluded are not identical.

*Sources. Textiles.* Unpublished chart 4 (Cotton textiles: U.S. Import Trends: Ratio of Imports to Apparent Domestic Markets), Office of Textiles, Market Analysis Division, Department of Commerce, February 1971; United States International Trade Commission (USITC), 1984, A-6. *Footwear.* USITC, 1985, A-15. *TVs.* USITC, 1980, D-6 for 1975-78; USITC, 1984, A-37 and A-105 for 1980-82. *Steel.* American Iron & Steel Institute, 1968-1980. *Autos. Ward's Automotive Report,* 1982, 1; "Imported Cars at 32.6% as Domestic Sales Fall," *New York Times,* 5 December 1985, D-1, City Edition. Data also received by telephone from the ITC and American Iron and Steel Institute.

the manufacturing average led both textile and apparel firms to step up their demand for protection (*Congressional Record* 1966, 20985).

Low profitability and rising import penetration gave legitimacy to the textile-apparel coalition's pleas for further protection. Hence, not only was the demand for protection higher, but the government became increasingly willing to supply it. With the foreign policy costs of non-negotiated protection still high, our model would suggest that the United States would expand the cartel to incorporate new players. The proposal by the Kennedy administration to extend textile restrictions in 1961 was therefore consistent with our expectations. The so-called short term arrangement, which was followed by a long term arrangement (LTA), sought to bring new players into the cartel.

Yet, given the industry's structure, even this cartel arrangement was not stable. New players continued to enter and import penetration continued to increase from 6% of the cotton market in 1960 to 12% in 1969 (United States International Trade Commission [USITC] 1978, C-3). More significantly, exporters shifted to unrestrained fabrics (wool, man-made fibers, and blends) to avoid the LTA. Imports rose rapidly—both in quantity and in value. From 213 million square-yard equivalents (SYE) in 1962, man-made-fiber-based imports rose to 1,453 million SYE in 1968. Imports of wool-based products rose from 141 to 210 million SYE in the same period. A similar rapid rise took place in apparel imports (USITC 1978, D-3).

Increased imports strengthened the unity of the textile-apparel coalition largely because the players had limited exit or adjustment options. Minorities and women dominated the work force, and more than two-thirds of the workers in textile and apparel factories were semi-skilled ("Textiles" 1982, 32c). This meant that labor had little job mobility. For firms, the barriers to exit were growing in textiles because of the increasing capital intensiveness of production. Economic barriers to exit remained low in apparel, but emotional and strategic barriers to exit left firms with limited mobility. Retailers were very large and powerful, precluding forward integration; and the fragmentation of apparel producers into small, single-product companies, made it difficult to expand into overseas production (Porter 1980, chap. 12). The vast majority of apparel firms (28,384 of 31,181) had less than 100 employees, and the top 30 firms accounted for only 7.4% of the industry's value added. Because many apparel firms were family-owned, they would choose divestiture only as a last resort.

While demand for protection continued to increase, resistance to protection on the part of the government waned. Despite protectionism, the industry's return on sales and return on equity rarely climbed above 50% of the manufacturing average. Sympathy for the industry became so strong within the U.S. government that officials were even willing to sacrifice some broader foreign-policy interests. Until the mid-1960s, for instance, Hong Kong had avoided stringent controls on its exports by implicitly threatening to close down the U.S. listening post for China. By 1966, such threats fell on deaf ears (Yoffie 1983a, 111).

The consequence of greater government willingness to supply protection and the increasing demands for trade barriers was the Multifiber Arrangement (MFA) of 1974. Subsequent renewals of the MFA in 1977, 1981, and 1986 transformed the regime into one of the most protectionist instruments of the postwar era. Yet despite the increasing number of textile VERs and the growing restrictiveness of the arrangements, the industry's low barriers to entry have made it impossible to create a stable cartel, especially in apparel. The restrictions simply failed to halt the decline in U.S. apparel employment or improve the competitive position of apparel producers. Japanese and East Asian producers have increasingly moved up-market into middle-range goods, while Association of South East Asian Nations (ASEAN) countries and other less-developed nations have taken over the lower end of the market. Some larger U.S. firms have adapted to import competition by engaging in outward processing while other apparel firms have become importers. Twenty percent of "American" apparel brands were imported in 1984.[9] Smaller firms that have not found higher-fashion, higher-price-market niches, have been increasingly forced to exit. The result is that the U.S. apparel industry ran a trade deficit of over $13 billion in 1984 (American Textile Manufacturers Institute [ATMI] 1985, 24).

The industrial characteristics of textiles have allowed manufacturers to become more competitive with imports. Textile employment has fallen by about 10% since the 1960s, but greater capital inputs have led to significant improvements in productivity. The textile industry, helped by cheap, regulated oil prices, even became a net exporter during the late 1970s. However, the deregulation of oil prices and the rise in the value of the dollar again squeezed the profitability and domestic-market share of textile manufacturers in the early 1980s.

In the aggregate, therefore, the industries comprising the textile-apparel coalition have experienced rising levels of import penetration and poor average profitability since the late 1950s, thus increasing the demand for protection. Barring large-scale exit from the textile and apparel industry or a new technology that would radically change industry structure, our model leads us to expect that institutionalized protectionism in this sector will become even more restrictive in the future.

### Temporary Protectionism: Color Televisions and Footwear

Although faced with competitive problems similar to those of textiles and apparel, the television and footwear industries did not succeed in securing steadily expanding protection. Instead, the VERs of the 1970s in both industries were allowed to lapse. The demand for protection in these sectors was not as strong as in textiles and apparel. Not only were these industries relatively small, but lower barriers to exit weakened the merits of the industry's case for protectionism and reduced corporate demand for trade barriers over time.

*Color Televisions.* Imports of television sets into the United States started to grow when Japanese companies captured a large share of the monochrome market in the mid-1960s (Millstein 1983; Yoffie 1983a). By the early 1970s, Japan made inroads into the color market, accounting for 15% of U.S. consumption in 1975. One year later, imports had surged to 33% of the market, Japan alone taking a 30% share (USITC 1980, D-6).

Pressure for protection had been mounting since 1970, when Zenith Corporation first filed a countervailing duty suit. Over the next seven years, various legal suits were filed by Zenith and other industry and labor groups, but none yielded any immediate benefits. The government continually refused to supply protection to this relatively small industry (40 thousand workers in 1973 and only 26 thousand in 1976). The Carter administration altered this position, however, when the International Trade Commission (ITC) recommended tariffs in response to a Section 201 complaint (the U.S. counterpart to GATT Article 19) and several of the law suits filed by Zenith appeared to be bearing fruit in the courts. To undercut this growing protectionist sentiment and preserve important foreign policy goals related to the Tokyo round of trade talks, the Carter administration negotiated a three-year OMA with Japan that began on 1 July 1977. The agreement limited Japanese exports to 1.56 million units per year and included a provision exempting sets in which 40% of the labor costs involved U.S. workers.

On the basis of our model, protectionism in this sector should be temporary, largely because the barriers to entry were moderate-to-low, the industry was small, and firms had viable adjustment options. By the mid-1970s, the color TV industry was relatively concentrated, with two firms—RCA and Zenith—accounting for 40% of the market. Other firms had liquidated their investments or sold their assets to the Japanese. Because all of the major producers of TVs except Zenith were diversified companies, divesting the unprofitable TV business did not mean life or death. Moreover, TV firms were large by textile and apparel standards and, in most instances, already multinationals. This made it relatively easy for firms to move offshore to reduce labor costs. In fact, RCA never joined in the battle for protection with Zenith precisely because it had already shifted much of its production offshore by 1977 and had licensed color television technology to the Japanese as early as 1962.

Barriers to entry in the industry were higher than in textiles and apparel, but surmountable. South Korea and Taiwan already had the necessary technology and were positioned to fill any gap in demand left by a reduction in Japanese exports. In 1978, Taiwan shipped 624 thousand sets (up from 235 thousand in 1976). Korean TV exports to the United States increased from 47 thousand in 1976 to 437 thousand in 1978 (USITC 1984, A-37). As a result, both profits and worker employment in the domestic industry continued to drop.[10]

The growth in imports led to only a temporary increase in protectionism. Within four months of the OMA, Zenith decided to close its domestic plants, move offshore, and lay off one-quarter of its labor force. This action decisively weakened the protectionist coalition. The major remaining supporters of protection were labor, firms that supplied TV components and, ironically, two Japanese firms—Sony and Matsushita.

Under the terms of the OMA, the United States was obligated to restrict new entrants if Japanese manufacturers were placed at a disadvantage. This led the Japanese firms producing in the U.S. to pressure the U.S. government for equitable treatment by negotiating OMAs with South Korea and Taiwan. The U.S. government therefore proceeded to sign agreements with these two countries in December 1978, with the accords to run from February 1979 to June 1980. By the end of 1979, the U.S. dropped the OMA with Japan but extended the ones with South Korea and Taiwan for two years. Both of these agreements were then allowed to expire in 1982.

Korea and Taiwan each increased exports by almost 250% after the OMAs expired. But despite the increased imports and low industry profitability, demand for protection did not rise significantly. By 1984, 17 producers were assembling televisions in the United States, of which 12 were foreign-owned (USITC 1984, A-8). Zenith, formerly the major force for protection, increased its overseas activities and diversification efforts, becoming in the process a more profitable company involved in the production of computers and computer accessories. As expected, then, protectionism in color TVs remained temporary: the government had little incentive to supply protection, and the relocation of production weakened the demand for trade barriers.

*Footwear.* The footwear industry, like the color TV industry, had been petitioning the government for protection for almost 10 years before it achieved any substantive results (Yoffie 1983b). When imports reached almost 50% of the market in 1977, the ITC recommended that tariffs be raised on nonrubber shoes. Faced with its first important trade case, the Carter administration decided to seek OMAs with Taiwan and Korea. It explicitly avoided the ITC recommendation for tariffs because it feared the transparent inflationary consequences. OMAs had the additional advantage of allowing the U.S. to avoid conflicts with important trade partners and allies, such as Italy and Spain.

The footwear industry, like textiles and apparel, has very low barriers to entry. Because the capital requirements were minimal and the technology has been stable, it would be a simple matter for other countries to increase their exports in response to the OMAs. Hence it was no surprise that imports increased by 60% from 1977 to 1978 and unrestrained countries such as Hong Kong and the Philippines increased their exports by 225% and 800%, respectively. If exit or adjustment options were constrained, as they were in textiles and apparel, we would predict that the industry's profitability would suffer, increasing the demand for protection. The government would also be sympathetic under these conditions, especially if it could minimize foreign policy costs by targeting the new, politically weak entrants, such as Hong Kong and the Philippines.

Yet this scenario did not occur. The critical difference between footwear and textiles lay in their industrial structures and the strategies available to major firms. The 20 largest footwear firms, representing about 50% of production, had viable exit strategies that allowed them to profit during the OMAs. Although profits of smaller firms suffered under the OMAs, large firms pursued strategies of forward integration into the highly fragmented retailing business and began importing footwear they could not make competitively. While manufacturers of less than one-half million pairs of shoes earned only a 4% return on sales (ROS) during the late 1970s, profits of producers of more than two million pairs rose from about 6% ROS before the OMAs to 9.4% by 1980. These differentials in performance split the industry and reduced the aggregate demand for protection when the OMAs came up for renewal in 1981.

On the supply side, the belief that protection should be temporary and terminated—once profitability was restored—undermined the case for further protection. Thus it is no surprise that the Reagan administration allowed the OMAs to lapse. One of the most interesting developments was that Korea and Taiwan maintained their own VERs after the U.S. opted for free trade in 1981. Because both East Asian governments had found advantages in the cartel (it allowed them to raise prices and pushed their producers into higher-value products), they continued to limit exports in order to keep prices up and to force domestic producers into the high-end markets.

Once protection was terminated, our model would predict that small firms would continue to demand trade barriers but that large firms would have less and less incentive to seek protection. This led us to the prediction that as long as large firms continued to exit and pursue profitable retailing and importing strategies, the government would not grant a second round of protection for the footwear industry.

Twice in the last three years, this prediction has proven accurate. Between 1981 and 1985, imports grew from 50% of domestic consumption to almost 80%. Nonetheless, the industry has been unsuccessful in its efforts to regain protection. In 1984, the ITC refused to recommend trade barriers in response to a Section 201 petition because large footwear firms had become more profitable than the manufacturing average. A year later, however, Congress changed the trade laws, partially in response to the footwear case. The new law dictated that the ITC could not reject a 201 case solely on the grounds of a single indicator, such as the level of industry profits. As a result, the ITC reversed its decision and recommended restrictions for the footwear industry in 1985. Yet the divisions within the footwear industry weakened the demand and the resources that the industry was able to mobilize. The president's eventual rejection of trade barriers was therefore understandable in light of our model.

### Sporadic Protection

Thus far, we have only looked at industries with low barriers to entry, where it has been difficult for the U.S. government to create a stable cartel that would help U.S. manufacturers regain their profitability. The steel and automobile industries, however, were both characterized by high barriers to entry. In such cases, we would expect negotiated protection to allow firms to achieve higher profits—at least temporarily. If this is the case, then protection should be liquidated because the government will be reluctant to provide protection for an industry no longer in distress. Yet unless an industry makes fundamental adjustments in these cases, this favorable position will rapidly erode and the industry may again seek protection. The U.S. steel industry illustrates this pattern.

*Steel.* The U.S. steel industry first faced import competition in the late 1950s. Imports captured an increasing share of the U.S. market, rising from 4.7% in 1960 to 12.2% in 1967 and surging to 16.7% in 1968. Growing imports and declining profitability led the industry to seek protection. In 1969, the United States negotiated three-year VERs with the two major exporters: Japan and the EEC. The U.S. preferred a negotiated solution because the costs of imposing a tariff or quota on such large exports of important allies were perceived as high. Although the negotiated agreements cut exports from Japan and the EEC by 25%, they provided enough flexibility to allow both importers and exporters to benefit.[11]

Unlike textiles and apparel, footwear, and TVs, steel had comparatively high barriers to entry, especially in the late 1960s. Capital and technology requirements were high, making it difficult for new entrants to capture any significant scarcity rents generated by the VERs. Because Japan and the EEC were the only viable exporters to the U.S. in the late 1960s, our model would predict that the agreements should have produced higher profits for U.S. firms. In the short run, the VERs did produce a drop in import penetration from 16.7% of consumption in 1968 to 13.8% in 1970, but the industry continued to suffer financially. The 1970 recession cut the average return on equity in steel to 4.3%, less than half the average of manufacturing (Crandall 1981, 29). This poor performance combined with an increase in imports from a few unrestrained European countries in 1971 strengthened the demand for protection and led the administration to extend the VERs in 1972 for three more years.

This time the agreements were more specific, and the cartel more effective. Imports dropped from 17.9% of the market in 1972 to between 12% and 13% over the next two years. Equally important, the steel market boomed and the U.S. industry's profits rebounded. Return on equity jumped to 16.9% in 1974, two percentage points *above* the manufacturing average. U.S. steel workers were also rewarded during this period. Between 1969 and 1975, steel wages went from 4% above the manufacturing average to 67% above average. This rebound reduced the government's willingness to protect the industry, and protection was liquidated at the end of 1974.

Had U.S. steel firms used their six years of protection to adjust or exit the industry, the industry would not have renewed its demand for protection. But the industrial characteristics of this industry made exit or significant adjustment very costly. As Crandall (1981) has argued, it has been pointless for U.S. manufacturers to reinvest in greenfield facilities. There was so much excess world capacity in steel that the discounted returns from such an investment would have been negative (Borrus 1983). The option of going abroad was also unavailable to U.S. firms because most countries encourage the development of indigenous steel industries. The only option for large integrated producers was to use the profits resulting from restraints to buy assets in more promising industries—a strategy that would take a long time to implement, given the huge fixed-asset base of all large steel firms.

Because the industry had made no fundamental adjustment (and, in fact, was less competitive after the VERs owing to high wages), imports increased, especially as new entrants in foreign countries overcame the capital and technology barriers to entry. They reached almost 18% of the market in 1977 as countries other than Japan, the European Economic Community (EEC), and Canada increased their share of U.S. apparent consumption from 1.2% to 4.1% of the market (Crandall 1984). The addition of 67 thousand steel workers to the unemployment lines between mid-1976 and mid-1977 plus a drop in average return on equity to close to the lowest among U.S. manufacturers raised the demands for protection once again (U.S. Congress 1977, 323). Politically, the industry was in a strong position to secure some form of import restraints. Various firms filed antidumping suits, which would have had high foreign-policy costs if the treasury found they had merit. In addition, the steel industry had very strong congressional support.

To head off costly protectionist legislation introduced in Congress, the Carter administration sought a negotiated solution that would share the burden of adjustment between the United States and its trade partners. Under Secretary of Treasury Anthony Solomon devised a solution called a trigger price mechanism (TPM), which set a minimum price based on Japanese production costs, below which there would be an automatic presumption of dumping with a threat of countervailing duties. President Carter pushed the EEC and Japan to accept the imposition of this system in return for the manufacturers dropping their antidumping petitions (Crandall 1981). By basing the TPM on the lower Japanese costs, it forced some of the burden of adjustment on the U.S. firms. It also allowed producers in the U.S., Japan, and the EEC to raise prices, as if it were a legal cartel.

However, an effective cartel could not last. Barriers to entry had effectively fallen by the late 1970s, making it harder to create a stable cartel and thereby raising the demand for protection. Because most major exporting countries in the world had developed a capacity to manufacture steel by 1979, the system of targeting protection against one or two countries became obsolete. By the early 1980s, the TPM was dropped, only to be replaced by a new VER with Europe. Despite occasional tactical maneuvers by the U.S. government and U.S. firms that have produced a few months of free trade in steel, trade restrictions have been maintained nearly continuously. By 1985, negotiated protectionism in steel had been expanded to all major exporting countries, making it look more and more like the system of protection in textiles and apparel. Not only has the demand for protection continued to be strong, but because most foreign governments subsidize steel exports, the U.S. government has been willing to intervene on the basis of "fair play" in international trade.

According to our model, steel should

continue to follow the path of textiles and apparel. Because barriers to entry have dropped over the past 15 years, a stable cartel has become unlikely. The only possibility that steel might follow a different path would occur if steel companies adjust or exit the industry. Both possibilities exist (Gutfleish, n.d.). The steel industry has become increasingly differentiated. Some producers have reorganized, reduced wage costs, and a few have become profitable with a greatly reduced product line.[12] The U.S. minimills—with their specialized products, modern electric furnaces, and labor that have not joined the United Steel Workers—have been able to compete successfully with imports. In 1980, minimills achieved a 14% return on equity—twice that of integrated steel producers (Walter 1981). In addition, some U.S. steel producers have begun importing steel products, and a few foreign firms have bought U.S. firms in the hope of evading protection.[13] Lastly, some U.S. steel companies have diversified out of steel production. Although steel firms have not been profitable, they have generated huge cash flow from the depreciation of their large fixed assets, which they have used to reduce their dependence on steel.

In sum, as more steel firms diversify and as minimills expand, the demand for protection could fall. As the industry splinters, its efforts to expand protectionism will be less and less likely to succeed. If, however, the diversification strategies falter, steel is likely to follow the path of textiles and apparel. The industry has the resources and the organizational strength to demand a high level of protection. Because the industry continues to be in distress and foreign countries subsidize their steel exports, the U.S. government is likely to regard protection as legitimate on grounds of fair trade.

*Automobiles.* The automobile industry provides a partial test to evaluate our model of the dynamics of negotiated protectionism. Because the barriers to entry have been high, VERs in autos should initially be successful in economic terms, leading to a reduction in protectionist pressure. Yet, as foreign producers outside of Japan overcome the barriers to entry, protectionist pressure is likely to reemerge. Unless auto firms exit the industry or make fundamental adjustments that improve their competitive positions, autos will follow a path similar to steel.

Historically, U.S. car manufacturers and U.S. auto workers supported free trade. Labor shifted its view toward the beginning of the 1970s, and the firms' position changed in 1979. In the wake of the second oil shock, U.S. citizens suddenly wanted small fuel-efficient cars, which Detroit was unable to provide. Japanese manufacturers filled the gap and Japan's market share jumped from 12% of consumption in 1978 to 20% in 1980. In 1979-80, the big three auto manufacturers closed 23 plants and sustained collective losses of four billion dollars. Auto unemployment increased to 26% in this period (U.S. Department of Transportation 1981).

By the end of 1980, the auto industry had formed a united front in favor of protection. The United Auto Workers (UAW), Ford Motor, and Chrysler led the charge, while General Motors, the firm best positioned to deal with the Japanese, reluctantly joined the coalition. Despite a rejection of a UAW-Ford Section 201 petition by the ITC, the industry used its considerable resources to pressure the administration to negotiate a VER with Japan in 1981. Like other cases of negotiated protection, the VER was designed to appease the domestic industry, avoid the transparent inflationary consequences of quotas, and avoid the high foreign-policy costs of directly violating GATT. The terms of the agreement represented significant give-and-take between Japan and the U.S.

Although the Japanese agreed to cut back their exports 8% from the previous year to 1.68 million cars, the agreement was extremely flexible and offered the Japanese opportunities to offset their potential volume losses.[14]

To understand the pattern of protectionism that would emerge in autos, we begin again with the barriers to entry, which in this case were extremely high. Not only were technology and capital requirements very large, but new firms require marketing and servicing capabilities to enter the U.S. Unless a foreign manufacturer enters into a joint venture with a U.S. or Japanese company to market its cars, it could take five or six years for a strong competitive threat to be mounted by new countries. Furthermore, other automobile manufacturers in unrestrained countries, such as Renault, Volkswagen, and Volvo, had a tiny share of the U.S. market, and they did not have the capacity to increase sales fast enough or in sufficient quantity to upset the stability of the cartel-like arrangement. Our model would therefore expect the cartel to be effective in raising the profitability of both the Japanese and the U.S. manufacturers, which in turn would reduce both the demand for protection and the government's willingness to supply it. As prices rose and profits increased, the legitimacy of protection for autos would weaken and firms would reduce their demand for trade barriers. The VER should have been allowed to lapse when the industry recovery took hold.

The VER had the predicted effects. Japanese manufacturers raised the prices on their cars, which allowed U.S. producers to increase their prices by an average of $2,000 per vehicle. Profits for the big three auto manufacturers rose from a $4-billion loss in 1980 to a $7.7-billion profit in 1983, and a $10-billion in 1984—their best year in history. Profit margins, including nonauto businesses, also rose from 1.73% in 1980 to 11.78% in 1983. These margins allowed the U.S. companies to generate badly needed cash flow. Working capital for U.S. auto firms increased from $0.9 billion in 1982 to $8.1 billion in 1984 (Auto and Truck Industry 1986, 101). Most Japanese auto manufacturers also achieved record profits in the United States during this period. Japanese profit margins averaged $2,000 per car under the VER in the U.S. market, which compared with $85–$125 per car profit in the Japanese market ("Profits" 1984, 68). The only unhappy Japanese producers were those with low exporting records before 1981: Mitsubishi, Fuji, Isuzu, and Suzuki would have preferred greater volume to the higher margins. Although Japanese car exports hovered between 1.7 and 1.9 million units from May 1981 to May 1985, the value of those exports rose from $8.8 billion before the VER to an approximately $12.4 billion in the year ending May 1985. This was especially impressive considering that the yen dropped in value by more than 10% during this period (Reagan 1985, 351).

Our model would lead us to expect that the record profits on both sides of the Pacific would reduce the demand for protection by U.S. manufacturers. When General Motors abandoned the protectionist coalition in 1985, it was only a matter of time before the VER would be dropped. Furthermore, the high level of profits and high costs of cars should have meant that the government would find little legitimacy in protecting highly profitable firms, especially with car prices rising. Thus, the decision by the Reagan administration to abandon the VER in May 1985 was consistent with our expectations. The decision by the Japanese government to maintain a VER, albeit at a higher level of exports, after President Reagan decided not to renew the agreement, was also not surprising. Because Japan prospered under the protectionist arrangement, the Japanese government and the large Japanese firms would want to maintain the cartel.[15]

Whether the auto industry will follow the route of steel depends in large part on the barriers to exit and available adjustment strategies. As in steel, the barriers to entry are increasingly being overcome by new entrants attracted to the high potential profits in autos. Yugoslavia and Korea both began shipments to the U.S. market in late 1985 and early 1986. What could differentiate the pattern of automobile protection from that in steel is that there are more avenues of adjustment open to auto manufacturers. First, capital is more mobile in automobiles: as in color TVs, auto firms can move their production offshore to reduce wage rates and increase their cost competitiveness. Second, improved automation, lower wage rates, higher productivity, and a substantially lower value of the dollar could also reduce relative costs to make U.S. firms more viable. Finally, enhanced design, quality control, and marketing capabilities could make U.S. companies competitive with the Japanese, even if there are cost differentials. Any combination of these options by the major U.S. producers could keep the demand for protection from rising.

In sum, if U.S. firms internationalize more of their production, and continue increasing their productivity, they may be able to fend off a new onslaught of imports, especially if they are aided by a continued fall of the dollar against the yen. The result would be another case of temporary protection. If, however, U.S. firms fail to improve their competitiveness, then the demand for trade barriers will again intensify, profits will fall, and protection is likely to be renewed.

## Conclusion

Our model helps us to understand the evolution of negotiated protectionism as practiced by the United States over the last quarter century. After its initiation, protectionism displays various patterns, which can be characterized as *institutionalized, temporary,* and *sporadic.* These patterns are accounted for not by conventional variables, such as import penetration, but by less-noticed factors such as barriers to entry and costs of exit from domestic production, as well as the ability of firms to adjust.

U.S. protectionism has become institutionalized in textiles and apparel because these large industries face low barriers to entry and their firms do not have attractive exit options. On the whole, such protectionism has been economically unsuccessful. Imports have continued to rise and industry profits have remained low. Temporary protectionism in the United States has appeared in industries that are relatively small (color televisions, footwear) and that confront low barriers to entry. Yet the ability of major firms in these sectors to devise effective exit strategies has meant that the economic failure of protection has not been translated into political success for protectionist forces.

Sporadic protectionism characterized the early years of trade barriers in steel. The economic success of import restraints resulted from high barriers to entry into the industry and led in turn to a decline in political support for protection. In the absence of effective adjustment by domestic firms, the ensuing decline in protection led to increased imports and renewed trade restrictions. Recently, barriers to entry to the steel industry have fallen and, as our model anticipates, protection has been extended to more countries and products, becoming increasingly institutionalized and rigorous. The history of the first auto VER in the early 1980s followed a similar pattern to that in the early years of steel: the economic success of protection led to a dismantling of restraints. Whether auto protection will turn out to be temporary or will recur depends in large measure on the effectiveness of the adjustment strategies pursued by domestic firms and the ability of non-Japanese foreign firms to overcome the barriers to entry.

Although our model does not allow us to make precise predictions of the trajectory of negotiated protectionism, it does provide some insight into this process. Negotiated protectionism as practiced by the United States has been neither linear nor cyclical but has followed more complex patterns. Economically ineffective protection often leads to political pressures for more extensive and highly institutionalized restraints on trade. By contrast, economically successful protection that increases profitability generates counterpressures that enhance the likelihood of protectionism's temporary demise. Protectionism's economic failures are often its political successes and vice versa. The dynamics of negotiated protectionism can only be understood by untangling the complex and dialectical relations between its political and economic components.

## Notes

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1. Despite formal legal differences between OMAs, VERs, and other types of negotiated protectionism, we treat these restrictions as functionally identical and will usually use the term VERs.

2. We include textiles and apparel in a single category not because they exhibit similar industrial characteristics but because they have successfully identified themselves as a single industry in political bargaining. The adjustment strategies and survival potential of the two industries differ significantly, as we indicate later in this paper; and it was not a foregone conclusion that they would get together in the first place (Aggarwal 1983).

3. Strictly economic judgments can also be complicated by political calculations. A strictly economic model incorporating rational expectations would hold that firms should not enter a market if they expect that future protectionism will make their investment unprofitable. Yet governments may decide to build textile or steel industries for reasons of security or national pride or because they expect positive externalities to ensue.

4. Maximizing firms, especially monopolists and infant industries, might anticipate future declines in profits and demand protection even in the absence of economic distress (McKeown 1984).

5. In our case studies, we use the industries' total revenues and employment as proxies for industry size. Since three of the industries are very large by these criteria and two are much smaller by an order of magnitude, our cases permit us simply to dichotomize this variable. A fully specified model would require more sophisticated measures. Revenue and employment data for our five industries at about the time of the first VER in the industry are as follows:

| | Employment | Revenues ($) |
|---|---|---|
| Textiles & Apparel (1958) | 1,842,274 | 25.9b |
| Steel (1969) | 436,400 | 22.3b |
| Color TVs (1976) | 26,967 | 2.1b |
| Footwear (1977) | 225,000 | 6.83b |
| Autos (1981) | 1,205,000 | 118.3b |

6. The *ceterus paribus* means no significant swings in the business cycle, no significant reduction in an industry's resources, and no significant exit by domestic firms. The latter two assumptions will be relaxed in the analysis.

7. Exit barriers are defined as "impediments to the removal of excess resources from an industry" (Caves and Porter 1976, 39). These impediments include not only fixed capital assets but also managerial and strategic impediments to leaving an industry. As Caves and Porter demonstrate, intangible factors, such as links to downstream and upstream operations (i.e., strategic barriers) and loyalty to a particular business or community (i.e., emotional barriers) may be just as significant as fixed investment in industry-specific assets.

8. It is conceivable that firms in an industry could use temporary protectionism as a "breathing space," to enable them to compete effectively with foreign industry, even without import barriers. This would also lead to a decline in pressures for protection.

9. Hank Gilman, "Clothing Shoppers Talk Domestic but Look First for Style, Savings," *Wall*

Street Journal, 15 October 1985, p. 35, Western edition. "Outward procening" refers to work performed abroad on materials exported from the United States that can be reimported with duty paid only on the value added, under section 8.07 of the U.S. Tariff Code.

10. The ratio of profits to sales in 1976 was 3.7%; by 1978 it had fallen to 1.5%. In the same period, employment in the industry fell from almost 27 thousand to under 24 thousand (USITC 1980, D-6).

11. The quantitative restraints pushed the EEC and Japan to produce higher-value steel and carve out market shares in higher-tech production. Although their import shares were cut from 8.4m and 7.3m tons respectively to 5.8m tons each, the value of their 1970 shipments was the same in 1970 as in 1968 (Adams and Dirham 1979, 98–99).

12. Daniel F. Cuff, "Forging a New Shape for Steel," *New York Times*, 26 May 1985, sec. F, pg. 4. Eastern edition.

13. Thomas F. O'Boyle and Doron Levin, "Wheeling-Pittsburgh Agrees on Venture in U.S. with Japan's Nishan Steel Company," *Wall Street Journal*, 8 February 1984, pg. 3, Eastern edition.

14. The United States accommodated the Japanese demands that trucks be excluded (which also meant the popular minivans that sold well in the United States in the early 1980s); that inventory estimated at four hundred thousand units be excluded from the quota; and, lastly, that Japanese producers would share in any growth in the U.S. car market after 1981 (Yoffie 1983a).

15. Japan was under severe pressure from the United States Congress, although not apparently from the executive, to maintain restraints. We attribute congressional pressure largely to concern about the huge overall U.S.-Japanese trade deficit, rather than to solicitude for the auto industry itself; this case therefore seems to provide support for our argument. If one viewed Japanese restraint as merely an informal continuation of the VER, in response to pressure from the U.S. automobile industry, this case could be regarded as an anomaly for our argument.

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# RESIDUES OF A MOVEMENT: THE AGING OF THE AMERICAN PROTEST GENERATION

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*The theory of political generations asserts that enduring and relevant political consequences result from critical experiences during the formative years. This study draws on a national three-wave panel study of young adults surveyed in 1965, 1973, and 1982 to test the theory with respect to the Vietnam era protest movement. College-educated protestors and nonprotestors are compared with themselves and with each other over time. Generational effects are categorized into absolute, relative, and equivalent continuity. Very strong continuities emerge for attitudes associated with the protestors' political baptism. Although erosion effects appear in more contemporary affairs, the protest generation remains quite distinctive. However, its limited size dampens the generation's political impact and qualifies the general thesis in a fashion that probably characterizes other examples of political generations also.*

Seldom has a youth movement swept a nation the way the student protest movement did the United States from the mid-1960s to the early 1970s. Beginning with a handful of dissidents, it quickly fanned out to embrace hundreds of thousands as active participants and sympathizers. While the movement had enormous political import at the time, the question remained as to its lasting effects. Certainly some of the institutional reforms spawned partly in response to the movement's demands remain in place. And the political memories of the era are deeply etched with images of the protestors and their actions.

Another kind of lasting impact, the effects on individuals caught up in the movement, is more problematic. At the time of the unrest, it was freely prophesied by some that the New Left politics of these unconventional activists would follow them to the grave. Other observers contended that the inexorable processes of aging and historical circumstance would bend and shape their politics into the mainstream, middle-of-the-road variety. More generally, however, the question was how this cohort, distinguished by its espousal of causes and its forceful efforts to achieve results, would fare as it marched through time.

The general rubric under which such questions are discussed is that of political generations. Mannheim's ([1928] 1972) classic essay on the topic sets the terms for the development of a political generation and its subdivisions, generation units: "Youth experiencing the same concrete historical problems may be said to be part of the same actual generation; while those

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groups within the same actual generation which work up the materials of their common experiences in different specific ways, constitute separate generation-units" (119). The generational thesis basically holds that the events experienced by youth during their formative years will have an enduring impact that is manifested in the political process. Thus, generational replacement becomes one of the engines of social and political change when and if new biological generations undergo novel experiences. Individuals coming of age during periods of pronounced stress and drama, epochal events, or rapid socioeconomic change are often said to be uniquely identified in a political sense—hence such labels as the "depression generation," the "silent generation," and the "protest generation" within recent U.S. history.

Modern survey research has not always been kind to the generational thesis (e.g., Cutler and Bengston 1974; Searing, Schwartz, and Lind 1973). In addition to the challenge of life stage explanations as a source of change, the thesis is confronted with historical or period effects in the form of broad-ranging social, economic, and political forces having an impact on all segments of the populace. Nevertheless, the theory remains attractive, in part because there are always enough striking examples at the extremes to suggest the wider existence of such phenomena. In many respects the so-called protest generation (or generation unit) provides an acid test of the thesis, for during its formative time it possessed strong political preferences, shared experiences, a common enemy, direct political action, and solidarity. If generational effects cannot be located among the protestors of yesteryear, the thesis suffers badly. If such effects can be identified, not only is the thesis supported but we can draw implications for the content and style of U.S. politics.

In using the term *generation effects*, by which, for convenience sake, we also mean generation *unit* effects, there is a tacit understanding that the long haul is being taken into account. Whether the approach is prospective or retrospective, analysts wish to establish the uniqueness of the group(s) under study as being of more than transitory significance. Continuity in outlooks and behavior is a common way of identifying a generation.

Three types of continuity can be defined. The first, *absolute continuity*, is most frequently what one has in mind. For example, a generation unit's response to the same attitude object must be roughly constant over time. The more fickle the response pattern seems to be, the less inclined we are to call it a generation. Note that the absolute continuity test treats the generation unit in isolation as it moves through time. Other generations and generation units may subsequently come to resemble the generation unit in question, thus erasing the distinctiveness. This is an important qualification of the absolute continuity test.

Absolute continuity is also, in some respects, a narrow and stringent test for it would demand an unchanging political and personal world. A second test of a generation's uniqueness, therefore, lies in its *relative continuity*. By this we mean the generation's ongoing position relative to other generations and the generation unit's ongoing position relative to other units in the same generation. From this perspective, relative continuity is a valid test. The question becomes, How distinctive does the generation remain with respect to the same type of political phenomena in the face of secular or other forces having an impact on all members of the population under study? Indeed this test is commonly employed in birth cohort analysis by detrending or adjusting the data to take account of general population movements.

Both of these tests assume relatively fixed political objects over time. As time passes, however, the political agenda changes, issues rise and fall, political personalities and groups come and go. If a generation is to remain recognizable, it should behave in a consistent and predictable fashion even though the specific political objects and practices involved are different from those of the past. A third test of generational continuity, then, examines the generation's response to new political stimuli not present at the generation's inception. We can think of this third test as *equivalent continuity.*

## Studying Generations

While Mannheim and others (e.g., Block, Haan, and Smith 1969; Braungart 1984; Feuer 1969; and Kenniston 1968) have lavished attention on the genesis and unfolding of youth movements and political generations, they have been less forthcoming about the subsequent lives of such groups. Contemporary empirical efforts emphasizing a longitudinal approach to the study of generations have been of two main types. One is the tracing of localized, easily identified individuals and groups that seem to have elements of generational phenomena attached to them. The various small-scale studies of student activists are of this genre (e.g., Fendrich 1977; Hoge and Ankney 1982; Nassi 1981; and Whalen and Flacks 1984). Although such studies may provide the fine texture of putative generations, they often suffer from parochialism and the lack of adequate comparison groups.

The second and more visible effort has occurred as analysts of large-scale survey data attempt to trace, by the rather indirect means of birth cohort analysis, the political histories of presumptive political generations (e.g., Abramson 1983; Baker, Dalton, and Hildebrandt 1981; Delli Carpini 1984; Holsti and Rosenau 1980; Inglehart 1977; and Nunn, Crockett, and Williams 1978). Valuable as these latter studies have been, they are limited by the coarse-grained, multipurpose materials of most national survey studies, the heterogeneity of general probability samples, and the weaknesses (offset by some advantages, to be sure) of repeated observations of the same cohorts rather than of the same individuals. Thus, most large surveys cannot easily isolate generations or generation units in the Mannheimian sense of the term.

The findings to be presented in this paper draw on a three-wave panel study of youths (and their parents). Beginning with a national probability sample in 1965, the study was extended in 1973 and again in 1982.[1] It is sufficient for present purposes to note that the unadjusted three-wave retention rate for the youth sample is 68%. Systematic analysis reveals very few differences between panel drop-outs and panel stayers. At the time of the original inquiry these respondents represented a probability sample of the high school senior class of 1965. Those who went straight on to college degrees became the college graduates of 1969. A look backwards reveals that this cohort lies dead center of the protest movement.

In reporting on the results from the 1965–73 panel, we took advantage of this historical timing by giving separate attention to those respondents who had been actively caught up in the movement, primarily in the form of its opposition to the Vietnam War. We concentrated on college graduates who had protested, partly because we wished to avoid the confounding effects of college training when comparing protestors and nonprotestors. Nearly 3 in 10 (N = 129) of all college graduates had taken part in at least one demonstration, protest march, or sit-in. Such a high rate of unconventional participation departed dramatically from that of the general population (e.g., Evans and Hildebrandt 1979 and, for an exception that proves the rule, Sniderman

1981). Analysis of attitudinal change during the panel period indicated that both selection effects and conversion effects were involved. Protestors were somewhat different prior to the protest era but dramatically different afterwards. Of course, not all of that change could be attributed to protesting and related phenomena, but multivariate analyses demonstrated surprisingly strong independent effects based on protest behavior.[2]

Since the third wave of the study was completed in 1982, we are in a position to assess the further history of the protest generation. Despite some further attrition from 1973 to 1982, we still have in hand 106 college degree protestors and 259 nonprotestors. The advantages of the data set over the small case studies are the presence of preprotest observations, a quasi-control group in the form of nonprotestors, and a representative sample. The advantages over national cross-section studies are the availability of enough cases of protestors for analysis purposes, a developmental rather than static design, and the capacity to perform both aggregate and individual-level analysis.

Two major clusters of political attitudes will be treated: 1) partisanship and civil liberties and 2) attitudes toward specific groups and issues. Absolute continuity in generational effects will be assessed mainly by observing mean or grouped tendencies over time. Relative continuity will be determined by comparing the protestors and nonprotestors at each point in time, recalling that the two groups are of the same age, have virtually the same level of education, and have shared the same general social and political history. Equivalent continuity will be given less explicit emphasis in this paper, appearing most directly in the section dealing with opinions on specific issues. Our analysis will be primarily at the aggregate level, for how the unit behaves in a collective fashion is the revealing test of generational residues.

## Partisanship and Civil Liberties: Baptismal Effects

We shall subsequently demonstrate that the liberal complexion of the protestors faded somewhat between 1973 and 1982 with respect to a large array of political attitude objects. However, that was by no means a universal experience. In two key domains the legacy of the protest experience was profound and persistent, and it is important to understand why this absolute and relative continuity prevailed.

### Parties and Elections

One of the preeminent ways in which conflicting values and goals are expressed institutionally is through the electoral system. Although protestors often railed against the establishment's conduct and used unconventional means of expressing their displeasure, it is significant that they did not abandon the electoral process. Indeed, they were more active in electoral politics than were the nonprotestors over the 17-year stretch covered by our study. In addition, their high turnout rate at the polls always matched or exceeded that of nonprotestors. Finally, it will be recalled that much protest activity took place within the realm of electoral politics, as witnessed by such events as the 1968 Democratic convention, the support given to New Left candidates within the Democratic party, the bitter anti-Nixon demonstrations, and the reform movement within the Democratic party in the early 1970s.

It is well known that early voting experiences, in addition to one's "inherited" partisanship, play a key role in shaping future partisanship and voting behavior. As an impressionable group that came of age at a time when its only acceptable and realistic political alternatives were in the Democratic party, it seems likely that the protestors would carry the Democratic imprint well into their adult lives. For all of its perceived shortcomings, the Demo-

**Table 1. Party Identification among Protestors and Nonprotestors, 1965–1982 (in percentages)**

| | 1965 | | 1973 | | 1982 | |
|---|---|---|---|---|---|---|
| Party Identification | Non-protestors | Protestors | Non-protestors | Protestors | Non-protestors | Protestors |
| Strong Democrat | 16 | 14 | 7 | 16 | 4 | 17 |
| Weak Democrat | 22 | 25 | 20 | 31 | 23 | 36 |
| Leaning Democratic | 12 | 26 | 19 | 31 | 16 | 21 |
| Independent | 10 | 6 | 12 | 14 | 10 | 7 |
| Leaning Republican | 10 | 7 | 23 | 4 | 17 | 10 |
| Weak Republican | 20 | 13 | 12 | 2 | 18 | 7 |
| Strong Republican | 10 | 9 | 7 | 2 | 11 | 2 |
| Total | 100 | 100 | 100 | 100 | 99 | 100 |
| Number of cases | 260 | 105 | 259 | 105 | 256 | 104 |
| Tau-c | .06 | | .33 | | .32 | |

cratic party provided the natural home for the protestors, if they were to have a party home. We shall demonstrate the extent to which this is true in two ways: 1) by looking at party identification as reported in the 1965, 1973, and 1982 surveys; and 2) by reconstructing presidential candidate preferences for the 1964–80 period.

To capture party identification, we rely on the standard questions used in the Center for Political Studies election series, questions that array respondents along a seven-point continuum ranging from a strong subjective attachment to the Democratic party to a strong attachment to the Republican party, with Independents being located in the middle. Somewhat surprisingly, the soon-to-be protestors left high school only marginally more Democratic than were the eventual nonprotestors (Table 1). In fact the combined figures for strong and weak Democrats are virtually identical: 39% for protestors and 38% for nonprotestors. Protestors were, therefore, not strongly self-selected in a partisan sense. Any differential affinity with the Democratic party exhibited subsequently would have to emerge as a result of later movements.

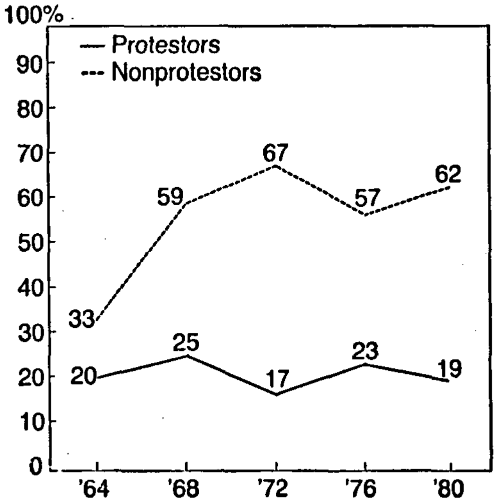
By 1973 both groups had shifted, but in *opposite* directions. Whereas the protestors became decidedly more Democratic (48%) the nonprotestors became somewhat less so (27%), gravitating instead toward Independent status. The large gain in the tau-c ordinal statistic showing the intergroup difference attests to the marked overall shift. Over the ensuing nine years, the two groups changed only modestly at the aggregate level and scarcely at all in terms of basic identification with the Democratic party. With several elections and untold political spectatorship behind them, the protestors by 1982 showed the indelible imprint of their political baptism. The critical shift came in the protest years, and the fixation came in the 1973–82 era, as they aged from 26 to 35.

Looking at the data from an individual basis shows this process in a different way. The over-time continuity coefficient ($r$) among protestors during the first panel period was .27, whereas it jumped to .53 during the second period. Given the slower rate of change in later life, there is good reason to believe that the protestors have crystalized their partisan attachment to the Democratic party and will carry it throughout their lives (Jennings and Markus 1984).

If protestors remain distinctive according to party identification, the same is more vividly so in terms of electoral behavior. During each of the three soundings, the respondents reported on their voting behavior and preferences in the preceding presidential elections. Putting the information together provides a track record for the five presidential elections reaching from 1964 to 1980. We will use reported preferences for those respondents who did not actually vote. There are two reasons for doing so. First, due to their age, scarcely any of the respondents could have voted in the 1964 elections, and a substantial minority were still too young even in 1968. Second, a comparison between reported voters and nonvoters reveals few differences in terms of the preference distributions. Primarily because the Anderson candidacy of 1980 affected the Democratic share of the vote, the results are reported in terms of Republican preferences (Figure 1).

Aside from blacks, it would be difficult to imagine a less-committed band of college-educated Republican voters than that represented by the protestors. Along with those who were not destined to become protestors, a minority of them supported Goldwater in 1964. But that proved to be a one-time experience. In every subsequent election, the nonprotestors rang up resounding Republican majorities. Meanwhile, no more than a quarter of the protestors supported the GOP candidate. As a result, what had been a very modest difference between the two groups in 1964 ballooned into a persisting one that never fell below 35% over the following four elections.

**Figure 1. Preferences for Republican Presidential Candidates, 1964–1980**



Again, the protest years are clearly implicated as the decisive ones. Both the 1968 and 1972 elections transpired while the protest movement was still alive and, if not well, at least kicking. In fact, the McGovern candidacy of 1972 was in many ways a consequence of the movement by the protest generation and other dissident elements within the party. Their voting behavior in the 1968 and 1972 elections appears to have virtually cemented the protestors to Democratic candidates, for even the moderate Gerald Ford was unable to capture much of their support in 1976. Individual-level analysis strengthens this view of persisting Democratic loyalties. Over the last three pairs of presidential elections (1968–72, 1972–76, 1976–80), the over-time correlations (*r*) were .46, .68, and .58 respectively. Comparable figures for the nonprotestors were .53, .47, and .49. Significantly, the protestors had the higher stability over the last two pairs of elections.

Elections have a way of reducing diverse political opinions and preferences to a very few choices. In the U.S. case, the reduction is severe. Within this context, the values and goals of the erstwhile protestors continue to be seen by them as being most closely approximated in the Democratic party. Had the protest era and the personal involvement of the protestors not occurred, it is by no means certain that this set of people would have become so wedded to the party. Forged in the fire of the protest years and annealed by the constrained choices offered by the

U.S. electoral system, the Democratic bias of the protestors seems destined to endure. However, the third-party candidacy of John Anderson in 1980 demonstrates the vulnerability of protestors to non-Democratic appeals. Whereas Anderson received but 7% of the total national vote and 10% of the nonprotestors' vote, he gained 23% of the protestors' support. But the Anderson candidacy was a rarity, and by 1984 the choice for the protestors was between Reagan and Mondale. There is little doubt about how protestors and nonprotestors were to divide their votes, though it seems highly likely that the protestors would have been solidly in the Gary Hart camp during the battle for the Democratic nomination.

## Civil Liberties

One of the earliest manifestations of the waves of student protest to sweep the country was the Berkeley free speech movement of 1964. In one way or another, civil liberties and civil rights became intertwined with a protest movement that had as its major focus the Vietnam War. Indeed, the civil rights movement, already well under way, often attempted to form coalitions with the antiwar movement, and vice versa. Because the legitimacy of the very act of unconventional behavior frequently hinged on a claim to first amendment freedoms, the protest movement was particularly sensitive to the classic questions of civil liberties. Of course, the passions of the demonstrators were sometimes said to infringe on the first amendment freedoms of other citizens. Nevertheless, the subscription in principle to classic doctrines of civil libertarianism seemed to form a part of the creed for all but the most revolutionary of protestors.

Previous analysis (Jennings and Niemi 1981, chap. 11) demonstrated the gap that had appeared between protestors and nonprotestors by 1973 with respect to beliefs about civil liberties. The question to be examined here is whether the protestors maintained their high commitment to the sometimes rather abstract principles of civil liberties. How did these commitments fare as social causes subsided and as worries about the health of the economy increased? And did nonprotestors continue to be less enthusiastic about these principles, thus keeping the distance fixed between themselves and the protestors?

Our first approach to these questions relies on a three-item measure dubbed the civic tolerance scale.[3] This index has four categories and, among college graduates, is highly skewed toward the upper, more tolerant end of the scale. Presented in Figure 2 are the proportions of protestors and nonprotestors scoring high—4—across the three surveys. It is clear that these high school seniors of 1965 destined to be part of the college protest crowd were only marginally more tolerant than those not so destined. Self-selection effects were minimal. Both groups had scored large gains by 1973, attributable in

**Figure 2. High Support for Civic Tolerance**

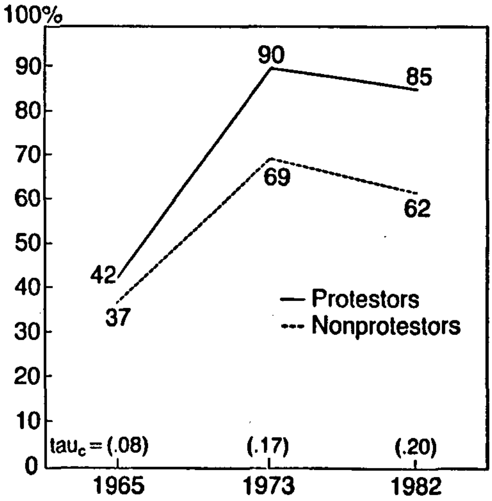
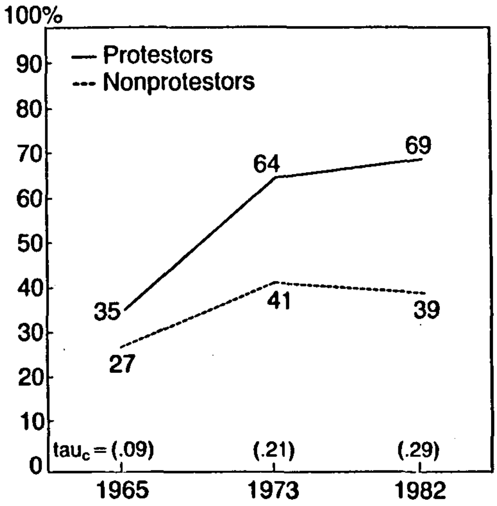


Figure 3. Opposition to Prayer in Public Schools



great part to their college training. However, the protestors achieved a massive gain. Nine years later, both groups had retreated just a shade, and the gap between the two had actually widened by a fraction. Overall, the protestors maintained both absolute and relative continuity.

A key part of civil libertarian beliefs in the United States is the separation of church and state. A number of historical battles have been waged over this principle. One of the recurring ones in recent history has been that of whether prayers (silent or aloud) should be allowed in public schools. During the Reagan years this particular issue has inspired considerable passion on both sides. Somewhat fortuitously, we began asking a question on this topic in 1965 and repeated it in 1973 and 1982. The results are presented in Figure 3, in terms of opposition to the concept of prayers in school.

Again, it should be noted that protestors differed very little from nonprotestors prior to the protest era. While both groups had increased their opposition by 1973, the rise was especially sharp among protestors. As with the civic tolerance measure, solidification seems to have set in at this point. Over the next nine years, neither group altered much in the aggregate. What change did occur saw the protestors becoming even more opposed to the idea and nonprotestors a shade less so. Consequently, the relative difference actually increased over time. The separation that had set in by the end of the protest era appeared to be a lasting one.

A final measure lies more in the realm of civil rights than of civil liberties as these terms are understood in the United States. Racial integration of the schools was for many years, including those of the protest period, a highly flammable issue. At one time it served as the litmus test for defining a civil rights advocate. Partly because they linked their own movement, spiritually if not physically, to that of the civil rights movement, it would be surprising if our cadre of protestors had not been early supporters of school integration. Of interest is whether that support has deteriorated in the wake of growing antipathy to the concept by the public at large.

The question employed asks about the propriety of the federal government's role in ensuring school integration. As shown in Figure 4, the nascent protestors were already more liberal on this issue than were nonprotestors in 1965, though each group had a solid prointegration majority. The protestors' support hovered around 75% at each of the three soundings, thus demonstrating extraordinary absolute continuity. An almost dogmatic quality is attached to their persistence because nationally the trend was plunging downward. Providing a dramatic illustration of this trend, the nonprotestors' support for integration dropped over 30% across the entire 17-year period. Not only was the relative position of the protestors maintained, it was greatly enhanced.

Taken together the findings on partisanship and civil liberties speak to the emergence of strong, enduring differences between the protestors and their same-aged, equally well educated fellow cohort members. Special circumstances surrounding their induction into political activism account in part for the staying power of the protestors' convictions in these two areas. The structuring of the electoral system helps perpetuate the partisan loyalties of the protestors, and the long-lived and rather classical character of the civil liberties domain helps perpetuate the protestors' high degree of liberalism. If one were looking for ammunition to support the theory of political generations, the partisanship and civil liberties arenas suffice in handsome style. But they provide only one of several possible outcomes, as will be shown.

## Group Interests and Policy Views: Erosion Effects

Our analysis of the 1973 data established beyond doubt the extraordinarily left-leaning quality of the protestors' political views with respect to a number of policy issues and group interests, both absolutely and relative to nonprotestors. For many observers of the protest generation the key question was whether they would carry their reform-oriented, liberal set of beliefs and values into their adult years. A politics contributed to by activists on the Left would be far different from one lacking that contribution. As they made their way through life, would the protestors remain true to their ideological wellsprings or would the press of the larger environment and the vicissitudes of everyday life make it all but impossible to preserve the rather extreme views of the early 1970s?

### Group Evaluations

A first approach to this question utilizes the evaluations accorded various socio-political groups on the so-called *feeling thermometer,* a device running from 0° to 100°. The higher the degrees, the more positively the individual feels about the object being evaluated. Nine groups were the subjects of questions in 1973 and 1982. Five of them—the military, big business, conservatives, Republicans, and the police—are generally identified with the more conservative elements in the polity. Protestors took a much more benign view of these groups in 1982 than in 1973. Although only the police managed to reach an average rating well above the 50° mark, all groups achieved substantial gains. One quick answer to our initial question, then, is that the protestors moderated considerably in their negativeness toward groups associated with the conservative establishment. In an absolute sense, they did retreat from their earlier positions, thus undercutting the notion of a persistent generation effect.

Yet absolute scores are only part of the story. Evaluations could rise, but the protestors might still be keeping themselves apart from other members of the college graduate cohort. Relative differences

**Figure 4. Support for Federal Role in School Integration**

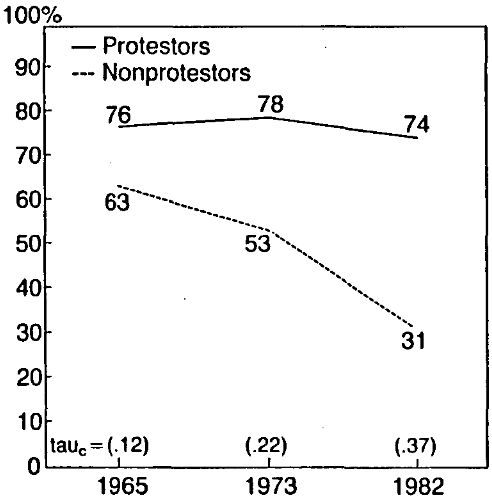


Table 2. Mean Ratings of Conservative Social and Political Groups

| Social or Political Group | 1973 Non-protestors | 1973 Protestors | 1973 tau-c | 1982 Non-protestors | 1982 Protestors | 1982 tau-c |
|---|---|---|---|---|---|---|
| Military | 49 | 28 | .43 | 59 | 42 | .36 |
| Conservatives | 54 | 39 | .40 | 58 | 48 | .28 |
| Big business | 44 | 31 | .35 | 47 | 45 | .08 |
| Police | 65 | 53 | .32 | 74 | 67 | .18 |
| Republicans | 56 | 48 | .31 | 58 | 50 | .23 |

Note: Mean ratings are based on the 0°–100° feeling thermometer technique. Tau-c coefficients are based on the full range of data.

would stay in place if the nonprotestors also upped their ratings. As it turns out, the nonprotestors also became more generous, thus keeping protestors relatively less approving of conservative groups. Because the increases among nonprotestors were less marked, however, the two groups grew closer together. This shrinking distance is expressed in summary form in Table 2 by the tau-c correlations, which show the relationship between protest status and group ratings. Protestors still stood out as being much less conservative, but time (and what it summarizes) had worked its will on them.

The marked mellowing of the protestors' views of establishment groups stands in contrast to their very modest increase in affection for four liberal groups (not shown). While protestors had felt more sympathetic to liberal groups in 1973 than had nonprotestors, the difference was far less than it was for conservative groups. This difference grew slightly by 1982, thanks in large part to the fact that nonprotestors failed to increase their evaluations of any liberal groups. Nevertheless, the gap between protestors and nonprotestors remained larger for conservative groups, a finding that suggests that the differentiated effect of the protestors had been and remains more anticonservative establishment than proliberal challenge.

## Inequalities in Influence

A basic complaint of protestors was that power was distributed so unequally in U.S. society. Along with other dissidents, they called for more representation of resource-poor groups in the decision-making process. They believed such changes were not only in the spirit of participatory democracy but would have the effect of improving public policies. At a very immediate level, the protestors clamored for more student power on their campuses and for more representation of youth in various institutions in the polity at large. To ascertain their perceptions and prescriptions about group influence, the respondents were asked to indicate whether they felt certain listed groups had too much, too little, or about the right amount of power. Table 3 presents the results for five social groupings that are widely perceived to be relatively powerless in an objective sense, or at least less powerful than their opposites.

Considering first the absolute changes over time, the protestors were universally less disturbed about the inequalities of influence by 1982. Even in the case of women, whose movement most closely approximates the spirit of the protest era, perceptions of too little influence dropped a shade. For the other groups the declines were more precipitous, reaching their

**Table 3. Attributions of Group as Having Too Little Influence**

| Group | 1973 Nonprotestors (%) | 1973 Protestors (%) | tau-c | 1982 Nonprotestors (%) | 1982 Protestors (%) | tau-c |
|---|---|---|---|---|---|---|
| Blacks | 37 | 78 | .37 | 32 | 61 | .27 |
| People on welfare | 36 | 63 | .25 | 26 | 50 | .25 |
| Young people | 38 | 65 | .24 | 36 | 36 | .01 |
| Women | 53 | 78 | .20 | 49 | 72 | .19 |
| Poor people | 74 | 92 | .15 | 66 | 79 | .12 |

Note: Percentages represent those respondents saying that groups have too little influence. Tau-c coefficients are based on the full range of responses.

extreme with respect to young people. If ever an argument could be made for a self-interest explanation of attitudinal change, the shift in ratings of young people is surely one of them. It may be true that the protestors, now in their mid-thirties, do not want the young people of today—who are most assuredly less liberal than the protestors—to have more influence. But it seems likely that life-stage progression, too, provides an explanation.

Nonprotestors had been considerably less distressed than were the protestors by the plight of resource-poor groups in 1973. They shared in the same general movement downward over time, but their changes tended to be less severe. As a result the difference between the two subgroups lessened decidedly in the cases of the poor, blacks, and young people. Both the absolute and relative singularity of the protestors were diminished.

It is worth noting that the same process applies to groups widely perceived as having too *much* influence. Whereas 94% of the protestors said in 1973 that big business had excessive influence, the same was true of "only" 78% in 1982. By comparison, the drop was from 87% to 79% among nonprotestors. On balance, then, erosion effects were at work across the board, acting to temper the singularity of the protestors. However, they still differed substantially from their nonprotesting classmates well beyond the protest era itself.

## Issue Orientations

What sparked the protest movement were issues—civil rights, freedom of expression, decision-making power, and, above all, the Vietnam War. With respect to the latter, it is hardly surprising that in 1973 over 9 in 10 of the protestors declared that the United States had erred in becoming involved in Vietnam. The figure had inched upward by 1982. Although a solid majority of the nonprotestors felt similarly, the proportion was lower, being 6 in 10 in 1973 and 7 in 10 in 1982. So strong were the protestors' feelings about policy outcomes in 1973 that two-thirds felt "a change in our whole form of government is needed to solve the problem facing our country." Only one-third of the nonprotestors felt likewise. Wide differences also separated the two sets of college graduates on a number of policy issues put to them in 1973. An especially crucial test by which to evaluate the generational quality of a cohort rests in the presence of consistent policy preferences over time. It becomes important, then, to observe any changes in the issue positions occupied by our respondents between 1973 and 1982.

**Table 4. Liberal Responses on the Self-Placement Scale**

| Issue | 1973 Non-protestors (%) | 1973 Protestors (%) | 1973 tau-c | 1982 Non-protestors (%) | 1982 Protestors (%) | 1982 tau-c |
|---|---|---|---|---|---|---|
| Liberal/conservative self-placement | 45 | 83 | .46 | 28 | 63 | .36 |
| Providing jobs | 29 | 62 | .39 | 13 | 36 | .28 |
| Legalizing marijuana | 55 | 83 | .38 | 34 | 61 | .31 |
| Rights of accused | 50 | 81 | .32 | 24 | 46 | .25 |
| Helping minorities | 47 | 78 | .31 | 24 | 53 | .28 |
| Equality for women | 77 | 91 | .24 | 84 | 90 | .16 |

Note: Percentage entries represent those respondents selecting the three most liberal positions on the seven-point scales. Tau-c coefficients are based on the full range of data.

One of the techniques employed for ascertaining issue orientations consisted of self-placements on issue scales running from one to seven, with each end anchored by a verbal statement describing the attitudes of people occupying that position. Five identical scales were employed in 1973 and 1982. A sixth scale was a general ideological continuum, anchored by *extremely liberal* on one end and *extremely conservative* on the other, with *middle of the road* being the center position. For present purposes, we have simply summed the percentages for the three most liberal positions on the scales.

At the absolute level, the protestors became decidedly less liberal over time (Table 4). On all measurements save one, they registered a decline of at least 20% in terms of the sum of liberal responses. What stands out in addition to the absolute reductions is the diversity of issues being tapped. Economics, civil rights, morals, and the criminal justice system are all represented here, as well as the encapsulating liberal-conservative dimension. Again, only in the domain of women's rights did the protestors show little retreat from their liberal stances of the past.

If the test of a political generation is absolute continuity at the aggregate level, then protestors do not pass with flying colors. They fare somewhat better when placed in a relative context. Nonprotestors in 1973 were far less liberal than their protesting peers. With one exception, women's equality, they too became more conservative over the next nine years. Similar forces—whether historical, life-stage, or both—were obviously affecting both groups. Still, the nonprotestors shifted less in an absolute sense. Protestors remained considerably more liberal but, as summarized by the tau-c correlations, they were no longer as different. Their claim to being a true generation (unit) had been substantially muted.

One of the difficulties with using an identical stimulus to tap political attitudes at different points in time is that the stimulus may not be equally salient at these several points. The meaning of the stimulus may have also changed over time (Sears 1983). For example, asking for an evaluation of Richard Nixon in 1982 was, for all but inveterate Nixon foes, asking about a different stimulus object than asking about him in early 1973. Another way of testing for generational strength is to determine if reactions to new stimuli follow a predictable pattern. Protestors should adopt liberal positions, both absolutely and relatively, with respect to newly emerging issues. If they do, we have generation effects in the form of equivalent continuity.

With these thoughts in mind, we asked several new issue questions in 1982 designed to tap attitudes about salient contemporary issues. One dealt with the issue of reducing government services in the area of human services, such as education and health. This was a prominent issue in the 1980 election campaign and continued to be featured throughout Ronald Reagan's first four years. Judging from their responses to the question, protestors still carry much of the liberal baggage from their earlier years. Whereas some 6 in 10 of them opted for the liberal position on this issue, the same was true of only 3 in 10 of the nonprotestors. Another question dealt with the highly emotional issue of abortion. While 73% of the protestors chose the most permissive alternative presented to them, 54% of the nonprotestors did likewise. Similar, though less marked, findings emerged with respect to the issue of the government's role in solving the problems of older people.

Our results based on employing more timely issues in 1982 are not dissimilar to those based on the replicated issue questions. Protestors definitely brought their more liberal political ideology to the "new" issues also. But the gap between them and the nonprotestors was, on average, less than that found on the issues in 1973 and more in the vicinity of the replications used in 1982. Policy differences remained, but the crystal-sharp definition of the early seventies had become a bit blurred.

## Methodological Artifact?

We have now established that the veterans of the protest movement maintained absolute and relative continuity in domains salient at the time of their political baptism but that they suffered both absolute and relative discontinuities in many other respects. One possible explanation for this turn of events is purely methodological. Given the extremely liberal scores of the protestors in 1973, the law of statistical regression might act to moderate their subsequent scores in 1982. The notion of statistical regression is that some portion of the extreme scores are not true scores. Subsequent assessments will find these extreme scores regressing toward their true values. Because the nonprotestors tended to have less extreme scores than did the protestors, they would experience fewer regression effects.

There are a number of reasons why regression effects are an unlikely source of our findings. In the first place, regression effects are not usually associated with general populations (or samples) grouped or categorized according to a priori criteria. Whether regression effects apply to groups defined according to an external criteria, in this case *protesting*, seems problematic. In a sense the extreme scores of 1973 constitute a validation of the respondents' classification of themselves as protestors.

Other points argue against artifactual results. Regression effects are usually associated with achievement and attitude inventories. As we discovered in a separate analysis of political participation, however, protestors on average also registered more declines in that area than did nonprotestors—though they still remained highly active by ordinary standards. Because participation is one of the more reliably measured of all political variables, it seems unlikely that the protestors' extreme scores of 1973 and subsequent drops could be attributed to measurement error. Yet another reason exists for doubting the role of regression effects. The section on partisanship and civil liberties showed high constancy (and even gains) over time among the protestors, even though as a group they had by far the more extreme scores at the time of the 1973 soundings.

Taking into account these various arguments, it seems most unlikely that statistical regression plays much of a role in the frequently observed gains in conservative political attitudes. There are plausible grounds for arguing that the protestors' more severe movements were anchored in "real" shifts. Moderating and countervailing influences could be expected to dampen more visibly the attitudes of individuals holding more extreme opinions. Moreover, the protestors looked upon themselves as having become more conservative during the 1973–82 period. Retrospective self-appraisals obtained through open-ended and issue self-placement scores revealed that the protestors far exceeded the nonprotestors in terms of ideological distance travelled since 1973 (Jennings 1984). Of course, they still remained far more liberal than the nonprotestors, but the protestors had a definite sense of having shifted their center of gravity to the right.

## Conclusion

Recent speculations in the United States have been directed toward the so-called baby boomers, those individuals born in the great postwar boom era and now in their thirties. Coincidentally, one portion of these baby boomers also contributed heavily to the protest generation. Baby boomers appear to be more conservative politically than what might be expected. Surveys show them giving large support to the Reagan candidacies and emphasizing economic issues, for example, though they are far from being conservative on social issues. The paradox being cited is the disjuncture between memories of the protest movement and this apparent conservatism. Our results speak directly to this seeming paradox; they also help us understand one way in which generation effects are preserved and continue to affect the political process well beyond their point of origin.

Our results address contemporary developments in two ways. First, they demonstrate that these baby boomers *did* become more conservative in a number of politically relevant ways over time. The more ideologically extreme segments, the protestors, underwent more movement than the less extreme ones. And this was true whether the test was one of absolute, relative, or equivalent continuity. Given the highly politicized nature of the protestors, there is little reason to think that these shifts are simply an expression of "what's in the air," all the more so because they could render reasonably articulate reasons for having become more conservative (Jennings 1984). Popular impressions had a basis in reality, though exaggerations abound.

Second, as more prudent observers always recognized, the protest movement was never a majority movement. Rather, protestors comprised a generation unit, as we have been at pains to point out. Even among our college graduates, who are themselves a minority of the class of 1965, the protestors constituted a minority. As our analysis of the partisanship and civil liberties data revealed, this minority proved spectacularly steadfast and distinctive with respect to issues and objects accompanying their political baptism. Numerically, however, the protestors are a minority within a minority. In this respect, then, there is no necessary paradox at all.

At a more general level, our results illustrate two points about the theory of political generations. First, generational persistence, while real, is subjected to stringent tests over time. In the present case, the residues of the protest movement were remarkably strong in some respects, thereby giving the lie to projections of quick assimilation. Nevertheless the gradual erosion of some attitudinal differences between protestors and nonprotestors indicates that over an extended period of time it becomes increasingly difficult to stay unique. Bearers of the generational ethos are vulnerable to the same general societal drifts and major events as are other segments of the population. They are also susceptible to life-stage demands that can undermine that ethos. As has been observed in a number of contexts, time often has a way of smoothing out certain prior differences within a population, including those of a generational sort.

A second general point is that the continued manifestations of a political generation may be quite localized but nevertheless quite potent. Trace elements may appear only sparingly among general populations, a not uncommon finding in surveys of mass publics. Among more politicized, passionate, and skillful subpopulations, however, the residues of the formative experience may be strong indeed (Converse 1986). Thus, if one listens to certain contemporary debates or observes political influentials in action, the imprinting of the generational experience is often patent. Because of their influence, elites and near elites can continue to represent the orientations of a political generation even when these orientations are not widely shared by mass publics. In this sense Mannheim's formulation is well-served, for he was basing his generalizations on the intensive analysis of politically active university students. Our protestors illustrate this formulation superbly because they have remained extraordinarily active politically. It seems quite likely that the same unfolding has characterized other historical instances of political generations. The theory of political generations needs to recognize and maintain the distinction between applications to mass publics versus more specialized yet critical groups. Each is important, but they denote different processes and outcomes.

## Notes

A version of this paper was presented at a conference on Youth in Western Democracies, November 1984, in Cadenabbia, Italy. I acknowledge the financial support of the National Institute on Aging, which sponsored the third wave of the data collection. I am grateful to Laura Stoker for her assistance and to Gregory B. Markus and Ethel Klein for valuable comments.

1. Details of the design and results from the first two waves are contained in Jennings and Niemi 1981; a first report of the three-wave study is found in Jennings and Markus 1984.

2. This topic is treated in some detail in Jennings and Niemi 1981 (chap. 11). Physical controls and statistical controls were used in order to test for the workings of other plausible explanations for the observed results. In particular, college major, school size, and public-versus-private status were used as physical controls. In addition, regression coefficients were calculated for the effect of protest behavior controlling for several background variables as well as several pre-1973 attitudinal variables. Conservatively, the protest experience could be said to be associated with dramatic behavioral and attitudinal differences in 1973 that were far less visible in 1965. Less conservatively, it could be argued that the protest experience helped *forge* these emergent differences.

3. The three items offered in an agree-disagree format ran 1) "If a person wanted to make a speech in this community against churches and religion, he should be allowed to speak," 2) "If a communist were legally elected into some public office around here, the people should allow him to take office," 3) "The American system of government is one all nations should have."

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# ELITE IDEOLOGY AND RISK PERCEPTION IN NUCLEAR ENERGY POLICY

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*Changing U.S. attitudes toward new technologies are examined, as are explanations of such changes. We hypothesize that increased concern with the risks of new technologies by certain elite groups is partly a surrogate for underlying ideological criticisms of U.S. society. The question of risk is examined within the framework of the debate over nuclear energy. Studies of various leadership groups are used to demonstrate the ideological component of risk assessment. Studies of scientists' and journalists' attitudes, media coverage of nuclear energy, and public perception of scientists' views suggest both that journalists' ideologies influence their coverage of nuclear energy and that media coverage of the issue is partly responsible for public misperceptions of the views of scientists. We conclude with a discussion of the historical development of the environmental movement in the 1960s and 1970s and the relation of this movement to the public's declining support for nuclear energy.*

Public attitudes toward the environment and the risks and benefits of technological progress have undergone a dramatic change in the United States during the past 20 years.

Today most citizens appear to be convinced that wide-ranging laws protecting them against risks and environmental degradation are absolutely necessary. A significant portion of the population contends they are willing to accept a drop in their standard of living (though they do not believe this will occur) to reduce pollution or other environmental hazards ("Environmental Update" 1982, 35).

The shift in public attitudes has been accompanied by a shift in the structure of power. Twenty-five years ago most decisions having to do with new technologies were made by the private sector or by the president, a few congressmen, and some bureaucrats, in consultation with the relevant industries and a small elite group of scientists. Today the business world enjoys much less credibility, authority, and freedom. Increased public-interest-group activity and media attention have been accompanied by expanded bureaucratic controls, much broader legislative involvement, and significant judicial intervention.

The scientific establishment is far less influential than it once was, and scientists employed by industry have less credibility than either Ralph Nader or scientists who are members of "public interest" groups. A 1982 national survey of public attitudes toward the electric-utility industry (Un-

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published Cambridge Reports, January 1982) reveals that only 12% of the population regard the testimony of a "consortium of public utilities" on energy matters as "very believable," and only 8% give that rating to the nuclear industry. On the other hand, 37% regard the testimony of "a scientist at a leading university" as very believable; Ralph Nader is voted very believable by 24% of the public; and "your favorite television commentator" is highly trusted by 17% of the sample. For these reasons, in part, various "public interest" groups now have considerable clout. Indeed, despite the efforts of the Reagan administration to loosen regulatory controls, such groups have won at least as many battles as they have lost.

What explains these shifts in public attitudes and in the structure of political power? The extant literature offers three broad answers to that question. First, several scholars have argued that the shifts are partly the result of economic and technical change itself (Aharoni 1981; Brooks 1979; Eisenbud 1978; Perrow 1984). Technological change has considerably accelerated since World War II. We now have the capacity to injure much larger populations much more easily than ever before in human history, either intentionally or inadvertently.

Of course, although potential dangers are greater, we are still becoming more affluent and healthier (Wildavsky 1980). Paradoxically, such progress has reduced citizens' willingness to accept risks. Our expectations have grown. In addition, scientific advances have enabled us to detect smaller and smaller traces of possibly toxic substances in our environment and to measure ever smaller effects of such substances on populations. Thus, once even minute traces of possibly toxic substances are found, the national media highlight the discovery, and administrative agencies move to regulate.

Second, critics of U.S. business practices argue that the anxieties of ordinary citizens about the environment and certain technologies are rational responses to real dangers. Ralph Nader and his followers, the staffs of some environmental groups, and other activist scientists such as Barry Commoner believe that through carelessness or self-interest, if not actual malice, U.S. business is dangerously polluting the environment with everything from nuclear energy to food additives (Commoner 1971; Efron 1984; Epstein 1979; Nader and Abbots 1977). Radical critics insist that many scientists and engineers exploit new technologies without considering consequences or seek to solve social problems by technological fixes. They level the same charges against conservative businessmen.

Third, some theorists deny that the development of technology is as threatening as critics imply. They maintain that other motives are involved in attacks upon the supposed dangers of technological change. Aaron Wildavsky and Mary Douglas (1982), for example, see "extreme" environmentalism as part of a new cult produced by the erosion of traditional U.S. values. They regard environmental issues as a surrogate for a broader attack on those values.

All the positions outlined above contain some truth. The argument from technical change has much to recommend it. The United States and other industrial countries are faced with real and serious problems. In addition, businessmen with financial and emotional commitments to new technologies find it very difficult to accept evidence of negative effects until such evidence becomes irrefutable. Of course, socialist managers exhibit the same reaction, which is why socialist nations have similar problems in dealing with environmental and public-health issues.

However, our own study of U.S. social, economic, and political leaders suggests that Douglas and Wildavsky are correct in arguing that ideological factors have also played an insufficiently recognized role in citizens' changing perception of risk.

Our argument consists of five interrelated hypotheses:

1. The relevant scientific community believes nuclear plants to be safe, and neither ideology nor economic self-interest plays a substantial role in their assessment.
2. Among other key leadership groups, by contrast, views about nuclear energy correlate with political ideology.
3. National media journalists are skeptical about nuclear safety.
4. Journalists inaccurately report the views of the relevant scientific community.
5. The views of other leadership groups on nuclear energy correlate highly with the news sources that they consider reliable.

These hypotheses are derived from a broadly speculative historical view of the growth of the environmental movement in the United States and its relation to changes in U.S. society.

We shall deal with these hypotheses more or less seriatum, comparing them to alternative hypotheses. We will then develop our historical analysis. We concentrate on nuclear energy because our research has generated sufficient data on this topic. However, we suspect that the same factors operate in other areas (Snyderman and Rothman 1986).

## Nuclear Energy, Ideology, and the Perception of Risk

In the 1950s, the vast majority of U.S. citizens supported the development of nuclear energy ("Opinion Roundup" 1979, 23). Today a substantial majority opposes the building of new nuclear plants. Moreover, opposition is strongest among the most articulate and politically active segments of the population. Cost overruns and the abandonment of many partially completed nuclear facilities in the United States have probably played a role in the development of negative attitudes among various leadership groups. Nevertheless, for the average person, fears about the safety are far more important (Inglehart 1984). The meltdown at Chernobyl in the Soviet Union has accentuated such fears to the point where nuclear development is not, at this time, a viable energy option in the United States.

The view that nuclear plants are unsafe is shared by a significant number of citizens in positions of social influence or responsibility. For the past several years, we have been surveying various leadership groups in the United States, including national-media journalists, science journalists, military leaders, congressional staff, partners in top corporate-law firms, creators of television and motion picture entertainment, high-level government bureaucrats, and public-interest-group leaders. The journalists were surveyed in 1989, although the follow-up questions on nuclear energy were administered in 1982. The remaining groups were sampled in 1982. The overall sample size for this analysis is 1,136 individuals. The samples were generally drawn randomly from each targeted group (see Appendix).

Respondents rated the safety of nuclear plants on a seven-point scale ranging from 1 (very unsafe) to 7 (very safe). We characterized scores of 5 or higher as exhibiting relative confidence in the safety of nuclear plants. The results (see Table 1) indicate considerable variation among the groups sampled. Thirty-seven percent of the journalists interviewed consider nuclear plants to be safe, as do only 13% of the television-entertainment elite, 14% of the motion-picture elite, and 6% of public-interest-group leaders. By contrast, 49% of the corporate lawyers believe nuclear plants are safe, as do 52%

**Table 1. Are Nuclear Plants Safe?**

| Sample Groups | Percentage Rating Nuclear Plant Safety 5 or Higher | Sample Size |
|---|---|---|
| Total leadership sample | 36.8 | 1,203 |
| Bureaucrats | 52.0 | 199 |
| Congressional aides | 39.1 | 132 |
| Lawyers | 48.6 | 149 |
| Media | 36.5 | 156 |
| Journalists at *NY Times* and *Washington Post* | 29.4 | 51 |
| Journalists at TV networks | 30.6 | 49 |
| Military | 86.0 | 152 |
| Movies | 14.3 | 90 |
| Public interest | 6.4 | 154 |
| TV, Hollywood | 12.5 | 103 |
| Total scientists sample | 60.2 | 925 |
| Energy experts | 75.8 | 279 |
| Nuclear-energy experts | 98.7 | 72 |

of top-level federal bureaucrats and 86% of military leaders.

How do these diverse estimates of nuclear safety accord with the scientific community's own evaluations? To find out, we sampled, in 1980, one thousand scientists drawn randomly from *American Men and Women of Science* (*AMWS*) supplemented by another sample of three hundred scientists who work in energy-related fields. We defined the latter group broadly, including persons in fields such as atmospheric chemistry, solar energy, conservation, and ecology. In all, we included 71 disciplines in our energy-expert subsample. In our smaller nuclear-energy-expert sample, we included experts working in such nuclear-related fields as radiological health and radiation genetics, as well as nuclear engineers and physicists. Seventy-four percent of the scientists sampled returned usable questionnaires.

Scientists, especially those in energy-related fields, regard nuclear energy as a necessary and relatively benign source of energy. Ninety percent of the random sample and 95% of the energy-expert sample believe that we should proceed with the development of nuclear energy. Seventy-six percent of the energy-expert sample and 99% of the nuclear-energy-expert sample believe nuclear plants to be safe. (For a full list of the results and the fields included in the energy subsample, see Rothman and Lichter 1982b; and Lichter and Rothman 1983.)

At least some critics of those scientists who support nuclear energy (Nader and Abbots 1977) have argued that the views of the energy-expert community are strongly influenced by political conservatism or economic self-interest. To test this hypothesis, a multiple regression analysis was performed on the energy-expert sample to predict scientists' attitudes toward nuclear power. We entered the following variables: employment status (business, government, pure academic, or consulting academic), region of residence, religious background, and self-described political beliefs. The regression (see Table 2) accounts for only 11.6% of the total variance in safety estimates, with ideology explaining only 5.3%. None of the other variables explains more than 1% of the variance in safety estimates, and scientists show no statistically significant

Table 2. Equations Predicting Nuclear Safety Attitudes Among Energy Experts

| Variables | Standardized Regression Coefficients Step 1 | Step 2 |
|---|---|---|
| Ideology | | -.27** |
| South | .09 | .05 |
| West | -.03 | -.03 |
| Midwest | .07 | .06 |
| Foreign | .03 | .03 |
| Protestant | .02 | .002 |
| Catholic | -.09 | -.10 |
| Jewish | .004 | .06 |
| No religion | -.10 | -.07 |
| Age | .02 | -.01 |
| Female | .09 | .09 |
| Government-employed | .11 | .10 |
| Business-employed | .17 | .11 |
| Academic consultant | .13 | .09 |
| $R^2$ | .053 | .116* |
| F-ratio | 1.09 | 2.37* |

Note: * = $p < .01$; ** = $p < .001$; N = 268

differences in their attitudes according to their type of employment. In short, scientific experts are mostly sanguine about nuclear safety, and economic self-interest plays no measureable role in their assessments. Ideology is a statistically significant predictor but the proportion of variance it explains is, as we shall see, very small as compared to that of nonscience leadership groups.

We administered a short form of the survey to the same respondents in 1985 with almost exactly the same results. A second replication of our study in Germany last year reveals that German scientists agree with their U.S. counterparts (Institut fur Demoskopie 1985). Engineers in energy-related fields are even more supportive of nuclear energy than are scientists (Lichter et al. 1986).

Our findings of discrepancies between expert and public attitudes are not unusual. Slovic and others have discovered that the public's perception of danger does not correspond to the judgments of experts or to actuarial tables (Slovic, Fischhoff, and Lichtenstein 1979; Upton 1982). Most significantly, they found that nuclear energy was rated as far more dangerous than any realistic assessment would indicate. They hypothesize that the public is more concerned with possible major disasters than with more familiar everyday causes of injury or death. No matter how remote the possibility of their occurrence, such disasters are perceived as far more likely to be highly disruptive, both socially and psychologically. The public can accept with equanimity that the number of deaths resulting from the burning of coal will be much greater than that which might accrue from the building of new nuclear plants because the former can be taken in stride (see also Perrow 1984).[1]

This hypothesis is not borne out by the evidence. As Wilson and Crouch (1982) and others (e.g., Aharoni 1981) have pointed out, people tend to ignore improbable highly destructive disasters and live their lives as if these would not occur. The probability of a worst-case nuclear meltdown is no greater than that of a large meteor striking a major city, a

**Table 3. Equations Predicting Nuclear Safety Attitudes among Leadership Groups**

| | Equation Number | | | | | |
|---|---|---|---|---|---|---|
| Variables | 1 | 2 | 3 | 4 | 5 | 6 |
| Alienation | -.46*** | -.23*** | | -.16*** | | -.15*** |
| Liberalism | -.24*** | -.42*** | | -.35*** | | -.34*** |
| South | | .005 | .04 | | | -.001 |
| West | | -.01 | -.01 | | | .03 |
| Midwest | | .04 | .07* | | | .04 |
| Foreign | | .01 | .02 | | | .03 |
| Protestant | | .07 | .31*** | | | -.02 |
| Catholic | | .06 | .24** | | | -.02 |
| Jewish | | .02 | -.003 | | | .002 |
| No religion | | .04 | .05 | | | -.0001 |
| Age | | .04 | .11*** | | | .05 |
| Female | | -.06* | -.18*** | | | -.06* |
| Bureaucrat | | | | .09** | .11** | .05 |
| Lawyer | | | | .07* | .10** | .04 |
| Television | | | | -.11*** | -.18*** | -.15*** |
| News media | | | | .04 | -.01 | .02 |
| Movies | | | | -.11*** | -.18*** | -.15*** |
| Public interest | | | | -.12*** | -.34*** | -.13*** |
| Military | | | | .19*** | .32*** | .16*** |
| $R^2$ | .41*** | .42*** | .17*** | .48*** | .35*** | .49*** |

Note: * = $p < .05$; ** = $p < .01$; ***$p = .001$; N = 1,063

major earthquake in California, a massive flood disaster in other parts of the country, or the collapse of a major dam. Yet people not only live with these possibilities, they even ignore them, failing, for example, to take out flood insurance.

How does one explain the wide differences in safety estimates among the various nonscientific leadership groups we have studied? Our second hypothesis states that, in contrast to the scientific community, social and political ideology plays a major role. To measure ideology, a factor analysis was performed on responses to social and political attitude questions. Respondents were asked to register scale agreement or disagreement with a series of statements of the form, "The American legal system mainly favors the wealthy." A principal-components analysis with varimax rotation identified the two factors shown in Tables 3 and 4. They were interpreted to represent socioeconomic liberalism and alienation from political institutions (see Appendix).[2] We then regressed safety attitudes onto the liberalism and alienation variables represented by the factor scores. The result appears in column 1 of Table 3. Ideology explains 41.2% of the variance in estimates of nuclear-plant safety. The contribution of liberalism is almost twice that of alienation, but both are significant at .001.

These ideological dispositions are not mere expressions of differences in social background, as we illustrate in columns 2 and 3. When a set of demographic variables representing age, sex, religion, and geographic region of origin are added to the equation, the change in $R^2$ is only .01, a statistically insignificant increment.

When the order of entry is reversed, social background alone accounts for 17% of the variation in safety ratings,

**Table 4. Equations Predicting Nuclear Safety Attitudes among Leadership Groups (Summary Effects)**

| | Variables | | | | |
|---|---|---|---|---|---|
| Step 1 | Step 2 | Column | $R^2$ | Stepwise Change in R | F-Ratio of Change |
| Ideology | | 1 | .41 | — | 371.53* |
| Ideology | Background | 2 | .42 | .01 | 1.80 |
| Background | | 3 | .17 | — | 22.00* |
| Background | Ideology | 2 | .42 | .25 | 226.22* |
| Ideology | Group | 4 | .48 | .07 | 19.32* |
| Group | | 5 | .35 | — | 81.41* |
| Group | Ideology | 4 | .48 | .13 | 129.66* |
| Group + Background | Ideology | 6 | .49 | .09 | 89.21* |

Note: Column statistics are beta weights. Liberalism, alienation, and age are continuous variables; all others are dummy variables.

* = $p < .001$

with age, gender and religion making significant ($p \leqslant .01$) contributions. Safety ratings are positively associated with increasing age, male gender, and non-Jewish religious affiliation. When the ideology variables are added, however, they not only increase the variation explained by 25% but also account for most of the variation previously attributed to age and religion. In the combined equation only alienation and liberalism make significant contributions at the .01 level, with gender also significant at .05. Thus, ideology clearly retains more predictive power than social background.

The independent influence of group membership on safety ratings is more difficult to assess, as the equations in Table 5 indicate. Owing to multicollinearity, we cannot exclude the possibility that the explanatory power of ideology overlaps with that of membership in one or another elite group. Dummy variables representing group membership significantly increase the variance explained in nuclear-safety assessments over the ideological variables alone, with $R^2$ rising from .412 to .479, an increment statistically significant at .001. In the equation shown in column 4, both ideological variables retain predictive power, but membership in the public-interest and Hollywood (television and cinema) elite samples also independently reduce confidence in nuclear safety, while membership in the military and bureaucrat groups increase safety estimates. When the stepwise sequence is reversed, in column 5, group membership alone accounts for 35% of the variance in safety attitudes, and ideology then provides a statistically significant increment of 13%.

Comparing these results suggests that ideology is the more important predictor because it explains more of the variance than group membership, both as the first and second entrant into the equation. Moreover, if either variable is primarily a surrogate for the other, the socialization and opinion-formation literature suggest that ideology would take preference over adult membership in an elite group. That is, broad sociopolitical orientations among highly educated and occupationally successful individuals tend to be formed early and to exhibit relative stability (Lichter, Lichter, and Rothman 1982; Ladd and Lipset 1975, 74–81; Mazur 1986).[3]

Further, with some exceptions, it is difficult to derive a consistently logical relationship between these occupational groups and attitudes toward risk. It might be argued that some groups (e.g., the military) are simply more willing to take risks than others (e.g., motion picture directors and producers). Yet that would not explain the assessments of many groups. For example, government bureaucrats and lawyers in large bureaucratic firms are not noted for their attraction to risk. Yet both groups are rather positive toward nuclear-energy development.

Concern about safety is the major determinant of support levels for nuclear-energy development among members of leadership groups. Such concern explains about 46% of the variance in support for nuclear energy among the groups we studied.

Finally, there is no evidence that liberals or radicals are by instinct more risk-aversive than are conservatives. During the fluoridation controversy of the 1950s, conservatives were far more concerned about risks than were liberals. They used many of the same arguments about uncertainty and possible danger that have been used more recently by opponents of nuclear energy (Mazur 1981). Today the French communists are strong supporters of nuclear energy. As one communist deputy put it in dealing with the controversies over nuclear energy, "The enemy of the workers is not nuclear power and science, but capitalists and management" (Golay 1980).

In short, the key variable explaining support for, or opposition to, nuclear energy for the groups we studied is safety. But concerns about safety correlate highly with scales of political ideology. Our data, then, provide a prima facie case for our second hypothesis. Attitudes toward nuclear energy in key leadership groups are related to broad social and political perspectives. We turn now to our third hypothesis, namely that some of these groups have persuaded significant segments of the public to share their skepticism concerning nuclear safety.

## Nuclear Energy: Scientists versus the Media

As we have already seen, the scientific community is highly supportive of nuclear energy, which they regard as a relatively benign energy source that should be tapped; and the views of independent scientists on environmental issues are held in relatively high esteem by the population at large. One would, therefore, expect the public and various leadership groups to follow the lead of the scientific community on this issue. In fact the public is relatively unaware of the views of the scientific community. In a 1984 national poll, almost 6 out of 10 respondents expressed the belief that those scientists who are energy experts are evenly split as to the safety of nuclear energy or consider nuclear energy unsafe (Unpublished Cambridge Reports, February 1984).

The general public is not alone in these views. Most of the elite groups in our sample seriously underestimate scientific support for nuclear energy. For example, 70% of television producers, writers, and directors believe that fewer than 65% of those scientists who are energy experts support the further development of nuclear energy. Indeed, like the general public, most of the leadership groups we studied perceive energy experts as sharply divided on the issues of nuclear energy. And this belief is associated with their views of nuclear safety. We found a negative correlation of .36 between estimates of the opposition of scientists to nuclear energy and the belief that nuclear plants are safe.

Why should the general public and the leadership groups we studied be so misinformed on this question? Part of the reason for erroneous public perceptions of

**Table 5. Scientists' Attitudes toward Traditional Scientific Norms (in Percentages)**

| | "Scientists should restrict public statements on science policy matters to areas of expertise"* | | | "Research findings should be accepted by professional journals *before* they are reported in the popular press"* | | |
|---|---|---|---|---|---|---|
| Response | All Scientists | Very Pro-nuclear | Very Anti-nuclear | All Scientists | Very Pro-nuclear | Very Anti-nuclear |
| Disagree strongly | 23 | 21 | 50 | 7 | 6 | 33 |
| Disagree somewhat | 22 | 21 | 25 | 12 | 13 | 17 |
| Agree somewhat | 30 | 29 | 17 | 33 | 33 | 33 |
| Agree strongly | 25 | 29 | 8 | 47 | 48 | 17 |
| Number of cases | 907 | 199 | 24 | 908 | 200 | 24 |

*For $x^2$ test $p < .01$.

the views of the scientific community on nuclear energy lies with the manner in which information about those views is communicated to the public by scientists themselves. The scientific community as a whole does not communicate with the general public. Rather, a somewhat smaller group of more visible scientists does most of the communicating (Goodell 1977), and this group seems skewed toward the antinuclear argument.

Our survey asked scientists both how many articles they had published in professional peer-reviewed journals and how many articles they had published on science policy questions for the more general public. We also asked whether scientists should keep public statements on science policy matters restricted to their areas of expertise and whether research findings should be accepted by professional journals before they are released to the popular press. We then compared the responses of the most antinuclear and pronuclear scientists with those of the entire sample.

The results suggest that antinuclear scientists reject traditional norms of scientific communication (see Tables 5 and 6). Twenty-five percent of all scientists

**Table 6. Scientists' Professional and Nonprofessional Publications (in Percentages)**

| | Articles Published in Academic or Professional Journals* | | | Science-Policy Articles in Nonprofessional Journals** | | |
|---|---|---|---|---|---|---|
| Number of Articles | All Scientists | Very Pro-nuclear | Very Anti-nuclear | All Scientists | Very Pro-nuclear | Very Anti-nuclear |
| None | 11 | 11 | 18 | 89 | 87 | 64 |
| One to ten | 29 | 30 | 41 | 10 | 13 | 32 |
| Eleven or more | 61 | 59 | 41 | 1 | 0 | 5 |
| Number of cases | 900 | 201 | 22 | 837 | 183 | 22 |

*For $x^2$ test $p < .05$.
**For $x^2$ test $p < .01$.

strongly agree that scientists should restrict public statements on science policy matters to areas of their expertise, compared to only 8% of the most antinuclear scientists. Forty-seven percent of the general scientific community agree that research findings should be accepted by professional journals before they are reported in the popular press, while only 17% of the antinuclear scientists agree. The responses of pronuclear scientists, by contrast, are almost exactly the same as those for the entire sample.

The publication records of the scientific community reflect these beliefs. On the one hand, 57% of our entire sample and 59% of our pronuclear sample had published more than 10 articles in academic or professional journals, compared to only 41% of the very antinuclear scientists. On the other hand, while only 11% of all scientists and 13% of highly pronuclear scientists have published articles on science policy in popular journals, the figure for antinuclear scientists is 36%.

More significantly, among scientists who have published on nuclear energy in professional journals, only 1 out of 10 believes that the possibility of an accidental release of radioactivity from reactors is a very serious problem. In contrast, among those who have published on nuclear energy only in popular journals, 4 out of 10 hold this view. Only 1.5 out of 10 in the professional-journal group believe that there are serious problems with the safety systems of nuclear plants, compared with 7 out of 10 in the popular-journal group. Lastly, more than 9 out of 10 in the professional-journal group are reasonably or very sure that we now possess the knowledge to solve the problems of nuclear energy, as against only 6 out of 10 in the popular-journal group. All these differences are statistically significant.

The publicly oriented group is small. One hundred and twenty scientists in our sample have published on nuclear energy. Of these, only 10 have written solely for popular journals. However, this suggests that between one and two thousand scientists listed in *AMWS* had written articles on nuclear energy for the general public without ever submitting their ideas to review by professional peers.

The relative skepticism toward nuclear energy of scientists who write only in popular journals may help explain why the public has a distorted view of the attitudes of the scientific community. However, this cannot be the full story. Given the absolute numbers, many more pronuclear than antinuclear scientists have published articles in popular journals. It is possible that antinuclear scientists publish in larger circulation periodicals, but our data point to another factor.

Elite journalists, including leading science journalists, are skeptical of nuclear energy. The most skeptical journalists of all are television commentators. On a "nuclear-support" scale, constructed from several survey items whose scores range from −9 to +9, scientists in energy-related fields score 5.10, while television reporters and commentators score −1.89 (see Table 7). Journalists employed by the national media are politically liberal, and, as with other groups, their political ideology correlates with skepticism about the safety of nuclear energy. Most importantly, these attitudes seem to be reflected in news coverage. We analyzed news coverage of nuclear safety from 1970 through 1983 in the *New York Times*, the three major news magazines (*Time, Newsweek*, and *U.S. News*), and the three commercial television networks (ABC, NBC, and CBS).

This content analysis of national media coverage found that a distinct antinuclear tilt started in the early 1970s on television and the late 1970s in print. Overall, the major print and broadcast outlets analyzed failed to report the views of the scientific community accurately. Antinuclear stories outnumbered pronuclear

Table 7. Support for Nuclear Energy among Scientists and Leading Journalists

| Leadership Groups | Score on Nuclear Support Scale | Number of Cases |
|---|---|---|
| Nuclear experts | 7.86 | 72 |
| Energy experts | 5.10 | 279 |
| All scientists | 3.34 | 741 |
| Science journalists | 1.30 | 42 |
| Prestige-press journalists | 1.16 | 150 |
| Science journalists at *New York Times, Washington Post,* TV networks | .47 | 15 |
| TV reporters and producers | -1.89 | 18 |
| Public television journalists | -3.25 | 24 |

stories by two to one on television and in news magazines. Judgments of particular safety issues were primarily negative at all outlets, reaching a two to one margin on television. Among nuclear experts cited in news stories, critics outnumbered supporters by more than two to one in news magazines and five to one on television (see Table 8 and Lichter, Rothman, and Lichter 1986).

Additional evidence is available from other sources. The Media Institute found that antinuclear experts and groups appeared on prime-time TV newscasts during the 1970s about twice as often as pronuclear sources (Media Institute 1979). By far the most widely quoted "independent expert" source on nuclear energy on television newscasts during the period of the study was the Union of Concerned Scientists (UCS). The UCS has been very critical of nuclear energy.[4]

The individual "expert" who received the most television exposure during the period of the Media Institute study was Ralph Nader, and the only academic sci-

Table 8. Media Coverage of Nuclear Safety 1970–1983 (in Percentages)

| Content | *New York Times* | News Magazines | Television |
|---|---|---|---|
| Story slant | | | |
| Pronuclear | 7 | 25 | 17 |
| Antinuclear | 10 | 46 | 42 |
| Neutral/balanced | 83 | 29 | 41 |
| Safety judgement | | | |
| Positive | 45 | 45 | 34 |
| Negative | 55 | 55 | 66 |
| Expert sources cited | | | |
| Pronuclear | 9 | 17 | 11 |
| Antinuclear | 7 | 40 | 62 |
| Neutral/balanced | 84 | 43 | 27 |
| Number of stories | 486 | 213 | 582 |

Note: Ten-percent random sample for *New York Times,* full universe for magazines, 50% sample for television.

entific expert quoted in the aftermath of Three Mile Island was Dr. Ernest Sternglass, a strongly antinuclear radiologist. Neither in the eleven-year period before Three Mile Island nor during the month after were pronuclear outside experts among the 10 top-quoted sources. The only pronuclear sources in the top 10 were spokesmen for utility companies and the nuclear industry. As we have seen, the public credibility of such organizations is quite low.

As our data indicate, many independent pronuclear scientists are available. Some of them, like Hans Bethe, are both well known and active in their support of nuclear energy. The views of such people would have been far more representative of the scientific community than those of Dr. Sternglass.

Physicist Bernard Cohen recently conducted a poll of academics in the radiation health field. He found that 91% believe that public fears of radiation are excessive, and about the same proportion believe that television and the press exaggerate its dangers. When asked to evaluate the scientific credibility of 19 scientists who write or speak about radiation, these experts gave Dr. Sternglass their lowest rating (14 on a scale ranging from 1 to 100) (Cohen 1983, 258–62).

It is conceivable that antinuclear scientists are more likely to seek out the media than are pronuclear scientists. However, we found that pronuclear scientists are no more likely to refuse to be interviewed by journalists than are their antinuclear counterparts. It seems equally likely, therefore, that journalists tend to seek out antinuclear scientists. Indeed, the elite journalists we surveyed were asked to list information sources they considered reliable on nuclear energy. They selected more antinuclear than pronuclear sources by a margin of over three to two (Lichter, Rothman, and Lichter 1986).

To be sure, as Combs and Slovic have demonstrated (1979), the media give far more play to "unusual disasters" (such as collapsing dams, tornados, or railroad accidents) than to daily occurrences that may kill or injure far more people. In addition, however, journalists are probably no different from anyone else in that, to help them understand issues that are both controversial and highly technical, they seek out sources they trust. And people tend to trust "experts" who share their own social outlooks.

During the 1950s' fluoridation controversy, the leading national media deferred to the views of the scientific establishment in ways that they no longer do. Certainly, reporters then were just as interested in disaster as they are now, and the science staffs of television or newspapers were neither larger nor better-trained than they are today. Today, however, journalists regard "antiestablishment" scientists as rather more trustworthy than mainstream scientists (Goodfield 1981).

It is hard to know to what extent media coverage actually influences the views of the larger public. In the recent past, most academic commentators have suggested that this influence is minimal. However, more recently, scholars such as Elizabeth Noelle-Neumann (1973) in Germany and Michael Robinson (1976) and Benjamin Page (Page, Shapiro, and Dempsey 1984) in the United States, have begun to build an impressive case on the other side.

It is not unreasonable to ascribe erroneous elite and popular perceptions of scientists' nuclear-energy views at least partly to media coverage, and other evidence adds to the plausibility of this conjecture.

We asked a number of our elite groups to rate the reliability of six national-media outlets (e.g., the *New York Times*, the television networks, and *Time*) and six journals of opinion (e.g., the *National Review*, the *Nation*, the *New Republic*). We found a positive correlation of $r = .38$ ($p < .001$) between the belief that the *National Review* is a highly reliable publication and the belief that nuclear plants are safe. The *National Review* is a conservative journal that strongly supports the development of nuclear energy. On the other hand, we found a negative correlation of $r = -.19$ ($p < .001$) between the belief that public television is highly reliable and the belief that nuclear plants are safe. Public television commentators were the most liberal and antinuclear group of reporters in our sample, scoring −3.25 on the nuclear-support scale. Overall, the reliability accorded to various media outlets explained 31% of the variation in estimates of nuclear safety.

Thus, the data also support our third, fourth and fifth hypotheses. It does seem as if journalists' skepticism toward the safety of nuclear plants affects the manner in which they report the news about nuclear energy to the public at large. Further, the U.S. public has probably been influenced in its perceptions by the manner in which the risks of new technologies have been dealt with in the media.[5]

Why have liberals and those on the left in this country been so critical of new technologies such as nuclear energy compared to leadership groups in France, where the Left, for the most part, supports nuclear energy? The concluding section of our essay offers a brief speculative answer to this question.

## The Perception of Risk as a Surrogate for Ideology

What follows is an attempt to understand some of the often unconscious sources of attitudes and attitude change. That some of the sources of an individual's perceptions are nonrational does not mean they are incorrect. The perceptions of all of us, liberals and conservatives alike, are influenced by nonrational factors.

We hypothesize that nuclear energy is a surrogate issue for more fundamental criticisms of U.S. institutions. To establish this hypothesis, it is necessary briefly to compare U.S. and European political development and examine the history and dynamics of the environmental movement in this country, especially as it relates to nuclear energy.

Few commentators would deny that, for most of its history, public political discourse in the United States (as against Europe) has been dominated by the "liberal-capitalist" ideology that defined its founding as a nation. So pervasive has this ideology been that opposition to it could barely be conceived (Hartz 1955; Lipset 1977, 1983). The United States has lacked a genuine conservative movement supporting throne and altar and longing to retain elements of a traditional feudal past. Nor has it ever developed a socialist tradition of any size. Liberalism has been so pervasive that even its sharpest critics have been largely unable to articulate a coherent alternative to it, even for themselves. As elsewhere, U.S. liberalism of the nineteenth and first part of the twentieth century was closely tied to the idea of progress, especially material progress (Nisbet 1980).

As late as the 1950s, the professional and intellectual communities in the United States strongly supported both technology and economic growth, as well as the economic and political systems.

The opponents of science and technology—those most suspicious of change—tended to be on the small town, fundamentalist, populist political right. In general the environmental movement during this period was still primarily concerned with the preservation of unspoiled wilderness areas or creation of national parks, a concern that had always differentiated it from its European counterparts. Leaders of such major groups as the Sierra Club were largely people who loved the wilderness and were active sportsmen and sportswomen (Dubos 1980; Faich and Gale 1971; Nash 1982).

This all changed in the 1960s, although the change had been in the making for some time (Bell 1976; Rothman 1978, 1984; Rothman and Lichter 1982b). The growth of industrialism and affluence gradually produced a new middle class of professionals who were not closely tied to the society's central economic institutions and new cadres of intellectuals critical of underlying U.S. values. Just as important, traditional religious beliefs and institutions that had served as the underpinning of the socioeconomic system were eroding. With that erosion, some deeper levels of emotional support for the system were being undermined. In short, key segments of the new middle class were gradually becoming alienated from major institutions and key cultural norms.

The events of the 1960s brought their alienation to a head. The war in Vietnam and the race issue enabled the New Left, a largely upper-middle-class movement, to mobilize a much larger number of initially less alienated citizens for particular purposes (Rothman and Lichter 1982a).

The ability of the more radical "new-class" intellectuals to attract a broader constituency was increased by the explosion of college populations and by the television-sparked communications revolution that began in the 1950s. As late as the 1930s, "progressive" ideas developed by the creators and dispensers of culture in New York, San Francisco, Los Angeles, and Chicago produced only faint echoes in small-town U.S.A. or in the ethnic ghettos of the major cities. By the 1960s, such ideas were being routinely taught at colleges and universities and transmitted nationwide almost instantaneously. Their format was more persuasive, and, given the decline of traditional institutions, their effect was much greater (Rothman 1978, 1979). Most elite journalists and academics were not fully supportive of the New Left, but a significant number of them resonated with at least some of its ideas (Ladd and Lipset 1975; Rothman and Lichter 1982a).

In the early 1970s, the winding down of the Vietnam War and the increasing complexity of the race issue led to a diminution of overt radicalism. Once again, those who wanted to change U.S. society in fundamental ways found themselves leaders without troops. Nevertheless, significant numbers of middle-class professionals remained highly critical of the system, and traditional values commanded less support from other segments of the population (Rothman and Lichter 1982a).

During the 1960s, a new awareness of the possibly dangerous ecological and health side effects of technological change also began to emerge. Most commentators date this awareness from the appearance of Rachel Carson's *Silent Spring* (1962), although the book did not begin to receive significant attention until later in the decade.

The environmental issue was among several on which those hostile to the dominant values of the society could focus. The problems were real, and they could be used to mobilize a much larger number of individuals looking for a meaningful cause to express their hostility to "corporate capitalism."

It was just this group—urban, sophisticated, and cosmopolitan—that sparked changes in the character of the environmental movement and came to control the older "public-interest" groups, even as they created new groups (Dunlap and Olsen 1984; Faich and Gale 1971; Scaminaci and Dunlap 1986; Webber 1982). Public-interest-group leaders were the most liberal, or Left, of all the leadership groups we studied. For example, 51% believe that the United States should move toward socialism, and 34% approve of Fidel Castro, compared to only 5% who approve of Ronald Reagan (Lichter, Lichter, and Rothman 1983b).

Joining them were a small group of highly articulate adversary scientists who are generally on the Left and share their hostility to business. To them, the real dangers to the environment and health posed by nuclear energy result from the operation of industrial capitalism itself. They have enthusiastically entered the public arena to combat this evil.

Barry Commoner, a "public" and "adversary" scientist stated their perspective forcefully at an antinuclear rally in 1979:

> Who owns the air? Who owns the water? Who owns the earth? . . . You know the right answer —we do, the people of the United States. But who controls our resources? The electric utilities, the oil companies—they decide whether we get radiation with our power. They decide whether we get carcinogens in our food. Who owns America? For whose benefit is this country run? For people or for profits? . . . We stand for . . . the public control of oil companies, for running this country in the interest of the citizens, not in the interest of profits. (Wolfe 1982, 243)

Other activist adversary scientists such as George Wald or Richard Lewontin have not been much less reticent about their alienation from U.S. political and social institutions (Rothman and Lichter 1982a, 430–31).

These beliefs are sincerely held. Those who adhere to them are not engaged in fighting nuclear energy because of a Machiavellian desire to mobilize the population for secret purposes. Nevertheless, our evidence suggests that some who argue that citizens are being exposed to large-scale dangers from nuclear energy are partly seeking surrogates for a more direct attack on the dominant values of U.S. liberal capitalism. In France, by contrast, the political Left does not require surrogate issues. It can still mount a direct attack upon the existing social and political order.[6]

Liberals and radicals are not the only ones who treat certain social issues as surrogates for a much broader sense of alienation. The political Right, for example, has repeatedly attached great symbolic value to such issues as internal communist subversion or the need for prayer in the schools. These issues serve as focal points for attempts to deal with the complex and confusing changes taking place in U.S. society. Political moderates also engage in such symbolic politics.

The articulateness and mobility of the middle-class activists who created the new environmental movement gave them distinct advantages over their opponents. They could easily gain the attention of the media because of their communication skills and because leading figures in the news and entertainment media sympathized with many of their views. The activists and media leaders are very similar in background and orientation (Lichter, Lichter, and Rothman 1983a, 1983b; Lichter and Rothman 1981; Rothman and Lichter 1982b, 1984).[7]

In France, decisions on nuclear energy have remained largely technical in nature and are, by and large, dealt with rather quietly by technicians, although decisions on issues of nuclear safety are widely publicized; and in England, the ties that bind together the scientific establishment, journalists, and political actors remain largely intact, though there has been some erosion in both countries. However, in the United States the situation has changed quite drastically (Gillespie and Johnston 1982; Wade 1980).

Drawing upon slogans of democratic participation, U.S. activists and "public-interest" groups began to participate in political and public forums about risk, especially nuclear risk. Their participation has been encouraged by legislation, administrative policy, and court decisions, which sometimes even mandate the participation of the "public" in decisions about technical issues (Nelkin and Fallows 1978).

Thus forums, debates, media reporting, and even television programs and motion pictures have gradually created a public image of a dangerously polluted United States, a potential cancer plague, and a threatening nuclear-energy establishment at a time when, for a variety of other reasons, public trust in various traditional leadership groups has already eroded (Lipset and Schneider 1983).

Today, a substantial segment of the academic social-science community is convinced that critics of contemporary technology are more correct than the scientific establishment (Lipset 1982). Large numbers of citizens are also willing to believe that the various ills that beset them may result from the activities of industry or government. Consequently, they have begun to demand protection and compensation. Such ideas have become so deeply embedded that they will change only quite slowly, if at all.

*It is clear that the irrationality of the* regulatory process in the United States has been partly responsible for cost overruns and has, in some cases, probably reduced rather than enhanced the safety of nuclear energy. In France, which copied U.S. technology, the cost of nuclear energy is significantly lower and the safety record significantly better than in the United States. In this country, the hypertrophy of complex regulations has both reduced morale and encouraged "going by the book" to meet regulatory requirements rather than rationally relating means to ends (Barkenbus 1986; Wade 1980).[8]

The problems nuclear energy has faced in the United States can not, however, be laid entirely at the doorstep of the media, activist groups, or bureaucratic regulation. In France, the nuclear-energy industry is highly standardized. All plants closely resemble each other. Thus regulators can build on common experience, and the approval of new plants is a relatively simple process.

In the United States, on the other hand, nuclear plants are built by architect/engineering firms under contract to individual utilities. While most plants share a family resemblance, many differ slightly from their predecessors and necessarily require a full set of new hearings and evaluations. Further, while a number of larger utilities developed experience and increased competence over time, many smaller utilities built no more than one plant, and information about the kinds of problems that might be faced was not widely disseminated until quite recently. Managers often viewed nuclear facilities as little different from facilities that relied on coal or oil. Moreover, utilities did overestimate the rise in the demand for electricity.

Further, for many years both the Atomic Energy Commission (AEC) and the nuclear industry refused even to consider legitimate safety concerns and did conceal from the public (and themselves) some of the real dangers posed by nuclear technology. They also made grandiose claims about its potential advantages ("electricity too cheap to meter") and its *absolute safety.* They maintained that a major accident was all but impossible and scorned anyone who was even slightly skeptical. At least some of the hostility engendered by the industry among a segment of the scientific community must be seen as the result of AEC and industry unwillingness to consider reasonable suggestions for enhancing safety (Del Sesto 1979). Yet the influence of the critics would have been relatively minor in the absence of the factors considered above.

## Conclusions

The argument undergirding this essay is that the new environmental movement in the United States is partly a symbolic issue. Some liberal or radical members of key strategic elites (especially the news and entertainment media and public-interest groups), who are alienated from the social and political system but live in an environment in which they cannot directly criticize the hegemony of liberal capitalism, are drawn to concrete issues that serve as a surrogate for such a critique.

The process of substituting such issues for a broader critique of the society is not necessarily a conscious one, and the issues chosen are often genuine, that is, problems with which the society must cope. They must be issues that can appeal to a larger, nonideological constituency if they are to be successful in arousing public support.

To test our argument, we chose the question of nuclear energy. It is impossible to test a broad-gauged argument of this kind directly. One can, at best, infer from it hypotheses that are subject to empirical falsification. We developed five such hypotheses: (1) the scientific community believes nuclear plants to be safe; (2) among key leadership groups, nuclear-safety views correlate with ideology; (3) national-media journalists are skeptical of nuclear energy; (4) journalists report the scientific community's views inaccurately; and (5) key leadership groups' views correlate with the news sources that they consider reliable. All these hypotheses were confirmed by the evidence.

Drawing upon standard historical materials, we then briefly outlined the historical analysis that underlies our view of the reasons for the fall from grace of the nuclear-power option in the United States. We concluded that the fears of citizens about new technologies—and about nuclear energy in particular—have emerged because of real problems that deserve careful discussion, thought, and decisions. However, the exaggerated concerns about possible risk, which have by now gripped significant segments of the population, are related to conflict among various leadership groups, a weakening of the social fabric of the society, a social mistrust that dates from the early 1960s, and a shift in the locus of influence and power in the United States.

## Appendix: Methodology

### The Leadership Sample

Samples of the eight leadership groups used in this study were generated in similar ways. Response rates varied from 61% to 95%.

1. The media sample was derived from internal phone directories and, in the case of news magazines, the names of individuals listed on mastheads. Staff members with responsibility for news coverage were chosen in consultation with knowledgeable people. A computer-generated random sample was chosen from this pool of names. The final sample size was 238. The response rate was 74%. The organizations sampled were the *New York Times*, the *Washington Post*, the *Wall Street Journal*, *Time*, *Newsweek*, *U.S. News and World Report*, and the news organizations at NBC, ABC, CBS, and PBS. A follow-up questionnaire containing questions on nuclear energy was mailed to the 238 respondents in 1982. One hundred and fifty journalists responded. The liberalism and alienation scores of the respondents were somewhat lower than that of the original sample.

   We based our sample of science journalists largely on the work of Sharon Dunwoody, "The Science Writing Inner Club: A Communication Link between Science and the Lay Public," *Science Technology and Human Values*, Winter 1980, 14–22. Of 82 respondents contacted, 42 responded, a response rate of 51%.
2. The sample of top-level bureaucrats was drawn randomly from the Office of Personnel Management's *List of Senior Executive Personnel*. Political appointees were excluded. Half the sample was drawn from "activist" agencies such as EPA, and half from

"traditional" agencies such as Commerce or Agriculture. The final sample size was two hundred. The response rate was 85%.

3. Congressional aides are a random sample of key staff aides, from a universe of about three thousand listed in the Washington Monitor's 1982 *Congressional Yellow Book,* cross-checked with the *Congressional Staff Directory.* Both committee and personal staffs were included. The final sample size was 134. The response rate was 71%.
4. The lawyers are a random sample of partners in New York and Washington firms defined as "large" (more than 50 partners) in the *Martindale Hubble* guide. The final sample size was 149. The response rate was 66%.
5. The military are a random sample of field grade officers from the Pentagon phone book and the National Defense University (NDU) class roster. Pentagon staff were general and flag officers from a universe of 1,073. The NDU sample was from noncivilian students mostly at colonel or commander rank and above. The final sample size was 152. The response rate was 77%.
6. The motion-picture sample consists of a list of 149 writers, producers, and directors of the 50 top-grossing films between 1965 and 1982, created from listings in *Variety* magazine. The final sample size was 91. The response rate was 64%.
7. The TV sample was drawn from an initial list of 350 names of the most influential television writers, producers, and executives. This was a reputational sample derived from industry sources with a minimum requirement that members had been associated with the development of two or more successful prime-time TV series. From this list, a random sample of 172 names was drawn. The final sample was 104. The response rate was 60%.
8. The public-interest-group sample was drawn from lists of (1) presidents and members of boards of directors of formal lobbying organizations and (2) attorneys in public-interest law firms. For group 1, the sources included *Public Interest Profiles* (Foundation for Public Affairs, Compiled by John F. Mancini, Washington, D.C.: Foundation for Public Affairs, 1979, 1980, 1981, 1982); *Washington Five* (Cory T. Grayson Jr. and Sally Stauton, eds., Washington, D.C.: Potomac Books, 1979); and *Encyclopedia of Associations* (Denise S. Akey, ed., 17th ed., Detroit: Gale Research C., 1982). For Group 2 the sources were *Public Interest Law: Five Years Later* (Ford Foundation and the American Bar Association, New York: Joint Report by the Authors, 1976) and *Balancing the Scales of Justice: Financing Public Interest Law in America* (Council for Public Interest Law, Washington, D.C.: Author, 1976). Knowledgeable individuals were also consulted. The sample was restricted to individuals affiliated with groups based in the Washington, DC and New York metropolitan areas. The sample size was 157. The response rate was 84%.

With very few exceptions, those interviewed reside or work in New York, Washington, DC, or Los Angeles. The media sample was interviewed in 1979, the other samples in 1982.

The companies responsible for the interviewing were Response Analysis Corporation of Princeton, NJ; Metro Research, Washington, DC; Depth Research, New York, NY; and Carol Davis Research, Los Angeles, CA. All of the interviewers were employees of the firms conducting the research, who received special training for this study. All attended an orientation seminar and conducted preliminary practice interviews. Response Analysis supervised the pretesting of the questionnaire.

## The Ideology Variables

The ideology variables are based on responses to the social- and political-attitudes questions. Respondents were asked to register on a four-interval scale their agreement or disagreement with a series of statements such as "The American legal system mainly favors the wealthy." The questions were presented to the subjects on cards, which were shuffled before each interview. The respondents read the question to themselves and told the interviewer the letter code and their response. A "no opinion" category was also available. A factor analysis of these data isolated two clusters that were interpreted to represent alienation from cultural norms and liberalism. The scales were developed by selecting questions that had factor loadings of .5 or higher. The variables used are as follows:

*Liberalism*

V142. "Less government regulation of business would be good for the country."

V144. "The American private enterprise system is generally fair to working people."

V147. "It is not the proper role of government to insure that everyone has a job."

V148. "Under a fair economic system people with more ability should earn higher salaries."

V149. "Lesbians and homosexuals should not be allowed to teach in public schools."

V151. "Our environmental problems are not as serious as people have been led to believe."

V159. "It is wrong for adults of the same sex to have sexual relations."

V161. "It is sometimes necessary for the CIA to protect U.S. interests by undermining hostile governments."

V220. "How much influence would you *like* business leaders to have?"

*Alienation*

V143. "The American legal system mainly favors the wealthy."

V153. "The United States needs a complete restructuring of its basic institutions."

V154. "Big corporations should be taken out of private ownership and run in the public interests."

V157. "The structure of our society causes most people to feel alienated."

V163. "The main goal of U.S. foreign policy has been to protect U.S. business interests."

The scales were calculated by summing the responses and dividing by the total number of questions used in the scale. The responses were reversed for some variables so that the scales ran in a consistent direction. Cases were excluded if any one of the variables was missing. Of the 1,224 cases in the sample, 1,063 were used in this analysis. After calculation, the scales were standardized with a mean of 100 and a standard deviation of 10.

## Notes

This essay is part of a larger study of social and political leadership in the United States of which Stanley Rothman is the director and S. Robert Lichter is the associate director. It is sponsored by the Center for the Study of Social and Political Change at Smith College, the Center for Media and Public Affairs, the Research Institute on International Change at Columbia University, and George Washington University.

1. Slovic offers several additional hypotheses that we regard as more plausible, but we shall not discuss them here. Other commentators have suggested that natural disasters are generally considered to be acts of nature and, thus, foster a general resignation. However, such an argument would not necessarily apply to bursting dams. We suspect that fears of radiation, regarded as an invisible danger that only manifests itself after a long period of time, play a key role in the public's unique concern about nuclear energy.

2. Lipset and Schneider (1983) document the sharp decline of public confidence in business during

the past 20 years. Interestingly enough, the decline is primarily in public perceptions of business activity in the abstract. The public is still positive about business when describing actual contacts with business firms. As Walter Lippmann pointed out (1922), the influence of the media on public opinion is greatest in those areas in which the public receives no immediate, easily checked feedback in its own experience.

3. We used a more complex measure of ideology for our nonscientific elite groups than for our science sample. However, for these groups, self-described political beliefs correlated at .50 with the alienation factor and at .66 with the liberalism factor.

4. Our survey asked scientists to list any public-interest groups with which they were affiliated. Only one from the random sample mentioned the UCS. Extrapolating from that sample, we estimate that fewer than two hundred scientists of the 130 thousand listed in *AMWS* were affiliated with the UCS at that time.

5. Some critics also argue that citizens have changed psychologically. Whereas economic growth was once their most important concern, they are now in the process of choosing new values, so-called postindustrial or expressive values, and are less concerned with growth than with the quality of life itself (Wachtel 1983). Drawing upon the work of Ronald Inglehart (1977), the Office of Technology Assessment's report on nuclear energy picked up these themes in discussing the reasons for the decline of support for nuclear power in the United States (Office of Technology Assessment 1984, 227–30).

Inglehart and his colleagues have administered a questionnaire to a number of leadership groups in Europe and the United States designed to measure such values. Respondents were asked to choose from a list of options which they considered most and least important as goals for their society. The goals included such items as "maintaining a strong defense" and "encouraging economic growth," as examples of "instrumental" or "bourgeois" choices, and "increasing community participation" or "creating a society in which ideas rather than money are most significant," as examples of "expressive" or "postindustrial" values. They have discovered, not surprisingly, that student activists are much more likely than businessmen to list "expressive" values as most important to them. Some scholars have found that antinuclear activists score higher on postbourgeois values than do control groups (Inglehart 1981; Ladd, Hood, and Van Liere 1983).

Our own analysis does not support such findings. We administered Inglehart's (1977) questions to our leadership sample with results very similar to his. Entertainment elites and public-interest-group leaders chose postindustrial values by margins on the order of two to one, while the military reversed that ordering. However, when these variables are added to the regression equation in place of the ideology variables, only 2.4% is added to the variance explained. While this addition is significant, it is much smaller than the increase caused by the ideology variables. Indeed, when ideological variables are included in the equation, the significance of instrumental and expressive values disappears. Thus while the expressive- or postbourgeois-attitudes factor may play a role among some activist groups that have been sampled, they do not seem to be of major significance among key leadership groups in the society.

6. Nelkin and Pollak (1981) argue that the inability of the French antinuclear movement to get off the ground has been primarily a function of the centralized bureaucratic nature of decision making in France. There is some truth to this, but institutional structures do not explain why the French Communist party is strongly supportive of nuclear energy, and the French Socialist party at least moderately so.

Originally, most large European Communist parties and most large (or governing) European Socialist parties supported the rapid development of nuclear energy, while small Communist parties (such as the Swedish Communist party) opposed it. The rise of the environmental movement and the establishment of such groups as the Greens in Germany has changed the picture.

As traditional political categories erode in the second half of the twentieth century, those alienated from the system seek new ones. Nuclear energy is a natural issue given widespread public anxiety about nuclear weapons. France remains an exception at least in part because of the success of its nuclear program.

Like the student movement in the 1960s, the environmental movement, in its current form, was initiated in the United States and brought to Europe via the U.S. media. Antinuclear activism in Europe took off only in the middle 1970s. Indeed, U.S. citizens contributed directly to such activism. Petra Kelly, a key founding figure of the Greens, is the daughter of a U.S. father and a German mother and was raised in the United States (Nelkin and Pollak 1981; Shell 1973).

7. Of course, our hypothesis could be reversed. The Left has argued that businessmen (and conservatives in general) seek technological answers to social problems. Thus, they support nuclear energy in the hope that economic growth will solve what are essentially systemic problems. Even if radical critics are correct, however, this would not mean that conservatives support nuclear energy as a surrogate issue. After all, U.S. conservatives assert quite explicitly that they support nuclear energy because they consider economic growth essential. Traditionally, the Left has shared this view. As late as 1962, the Port Huron Statement of Students for a Democratic Society argued that nuclear energy would bring about sufficient affluence to power the

transition to socialism.

One should not seek nonrational "symbolic" explanations for beliefs unless rational-instrumental explanations do not suffice. Our findings indicate that conservatives' perceptions of scientists' views of nuclear energy are considerably more accurate than are those of liberals. This suggests that their views on *this* issue are less distorted by ideological perspectives than are those of the Left.

8. Also Matthew Wald, "Weakness in Nuclear Regulatory Program Cited," *New York Times,* 7 July 1986, sec. B.

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# POLITICAL INSTITUTIONS AND VOTER TURNOUT IN THE INDUSTRIAL DEMOCRACIES

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*Differences in voter turnout among industrial democracies are a function of political institutions and electoral law. Specifically, the presence of nationally competitive electoral districts provides incentives for parties and candidates to mobilize voters everywhere, thereby increasing turnout. Disproportionality in the translation of votes into legislative seats provides a disincentive to voting, which lowers turnout. Multipartyism assigns elections a less decisive role in government formation, depressing turnout. By generating more decisive governments, unicameralism provides a clearer link between elections and legislation, increasing turnout. Finally, mandatory voting laws produce a disincentive to not vote. Empirical analyses of average voter-turnout levels in the 1970s and 1960s across 19 democracies are consistent with these expectations, although Switzerland and the United States appear to be outliers. The results have major implications for the way we interpret national differences in voter-turnout rates.*

Political participation can assume a variety of forms including protesting, voting, and engaging more actively in campaign activities (Huntington and Nelson 1976; Verba, Nie, and Kim 1978). In the industrial democracies, however, more people vote than engage in any other type of routinized mass political behavior.

At the same time, there is considerable variation across the democracies in voter-turnout rates. For example, average voter turnout in the 1970s was 44% in Switzerland, 54% in the United States, 72% in Japan, 82% in Norway, and 94% in Italy (Powell 1986, 38). How can these differences be explained?

One common answer is that even though all of these countries are democratic, they differ in political culture. That is, they differ in terms of their citizens' "subjective orientation to politics" (Verba 1965, 513. See also Pye 1972). Some countries are said to have more participatory cultures than others (see, e.g., Almond and Verba 1963). Where cultures are more participatory, citizens display heightened enthusiasm for politics: they exhibit greater political satisfaction with and pride in their institutions and are generally more efficacious in the role they and their fellow citizens play in politics. According to this line of thought, cultures that foster such participatory values enhance voting turnout.

Some go even further to take high voter-turnout rates as prima facie evidence of participatory norms (see, e.g.,

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Putnam et al. 1983, 63-65). But if norms are defined in terms of a behavioral outcome, then those norms cannot logically be used to account for that outcome. Thus, the equation of participatory norms with turnout does not contribute to an explication of national differences in turnout.

However, the cultural explanation of turnout does draw attention to the fact that the way we account for turnout bears on the meaning we place on voting itself. For example, much popular discussion of the comparatively low turnout in U.S. elections in recent years has been cast in terms of values: low rates of participation are seen to reflect apathy or alienation, and it is commonly believed that higher turnout rates would indicate a broader public satisfaction with political life. At the same time, there is a venerable literature claiming that low participation may testify to satisfaction, and that high turnout is undesirable. What is distinctive about such explanations, of course, is their stress on values.

An alternative to the cultural explanation of voting turnout centers on institutional factors. This alternative has found particular favor among students of U.S. politics, reflecting in part the distinctiveness of U.S. electoral law (for an interesting early statement, see Gosnell 1930). Thus, Rusk (1970) documented the electoral effects of the introduction of the Australian ballot in the United States. And until the Voting Rights Act of 1965 and its amendments (especially that of 1970), procedures like poll taxes, literacy tests, and lengthy residency requirements inhibited voter turnout considerably. Even with the reforms that came with the voting rights legislation, electoral laws in the United States (such as the common 30-day residency requirement) continue to serve as a strong damper on voter turnout (Wolfinger, Glass, and Squire 1985; Wolfinger and Rosenstone 1980). Below the federal level, turnout in gubernatorial elections has been found to be substantially influenced by such factors as electoral law and the nature of political competition (Patterson and Caldeira 1983). Most recently, Powell (1986) has shown that while U.S. citizens seem to be highly positively disposed toward participation, voter turnout is much lower in the United States than in most other democracies, which he attributes to the U.S. legal and institutional environment.

That voter turnout should respond to institutional patterns makes good intuitive sense. Voting is everywhere systematically governed by laws and institutional arrangements that vary markedly from nation to nation. In contrast, the right to discuss politics and contact officials is recognized in all democracies in a uniform manner, and the right to protest is tolerated in most. Because it is an institutionalized form of political behavior, then, the volume of voter turnout would seem potentially more responsive than other varieties of mass political behavior to laws and procedures. If this is the case, then national contrasts in turnout reflect national variations in the institutionally induced incentive structures that confront voters, as opposed to differences in culture.

My purpose is to demonstrate that different institutional arrangements have a major and predictable impact on national rates of voter turnout. Part of the argument has already been offered by Powell (1986), and this paper builds on his work. Powell's principal purpose was to evaluate turnout in the United States by comparing it with the other industrial democracies. Even with the United States and Switzerland (the other deviant case) excluded, however, there remain considerable differences in rates of turnout, from 68% in Canada to 94% in Italy during the 1970s. My goal is to account for these differences.

## The Issues

It is scarcely novel to observe that political institutions shape the distribution of incentives for political actors, whether they are candidates for office or simply citizens contemplating whether to vote. My working assumptions are as follows: When there are incentives for candidates and parties to mobilize more voters, they will do so, and this will increase turnout. At the same time, I assume that institutional arrangements influence the degree to which potential voters think their vote will make a difference both to the election outcome itself and to the subsequent formation of a government. Five specific institutional factors seem particularly germane to turnout.

### Nationally Competitive Districts

For leaders, the relevant issue is the structure of political competition. Powell (1986) addressed this question in two ways. First, he examined the closeness of elections, on the grounds that close elections may give citizens more reason to vote and give parties more reason to mobilize voters. Second, he looked at the extent to which electoral districts are nationally competitive. He reasoned that

> where the chief executive is chosen by simple majority or plurality vote, all regions should be equally important (e.g., France). In countries where the chief executive is chosen by the legislature, as in the various parliamentary systems, the question becomes the nature of the constituencies electing the legislators. With proportional representation from the nation as a whole or from large districts, parties have an incentive to mobilize everywhere. With single-member districts, some areas may be written off as hopeless. (1986, 21)

The argument ties in nicely with Gosnell's (1930) analysis of turnout in Europe. Gosnell's data from the 1924 British parliamentary election showed that turnout was much higher in those constituencies where the margin of victory was close. From this pattern, he concluded that "the character of the British system of representation does not favor a high participation in every election district. In those constituencies in which one party is sure of victory, many electors regard voting as useless. . . . The single-member-district system of representation discourages voting in one-party districts" (1930, 14).

Powell's empirical results indicate that the closeness of elections has no bearing on turnout but that nationally competitive districts produce higher rates of turnout than do single-member districts. That electoral closeness has little effect may reflect an ambiguity in this variable as a measure of competition—it is not clear that frequent party turnover needs to occur before voting systems can be said to be competitive. Instead, the *potential* for turnover may be more critical to the degree of competitiveness. The latter, however, is difficult to define and measure unambiguously.

In contrast, Powell found that nationally competitive districts do appear important to turnout. This result is consistent with Gosnell's argument that the presence of such districts provides incentives for parties and candidates to mobilize voters, which increases turnout.

### Electoral Disproportionality

In large modern democracies, it is unlikely that any one vote will be decisive in shaping the overall outcome (see, e.g., Riker and Ordeshook 1973 for a discussion of the implications of this fact). Nonetheless, some electoral systems raise the potential decisiveness of the marginal vote. The degree of proportionality in the translation of votes into seats in the lower legislative house is important in this regard. Most electoral systems produce a degree of disproportionality in favor of the largest parties, but some systems generate a good deal more than others

(Rae 1971a). Highly disproportional systems require minor parties to accumulate many more votes to achieve a given degree of legislative representation, which lowers the benefits of voting for the supporters of those parties. The greater the disproportionality, then, the more likely are the votes of minor-party supporters to be wasted.

Disproportionality thus introduces a strong disincentive to potential minor-party voters' casting a ballot. At the same time, electoral disproportionality influences the strategies of parties and their candidates. Most notably, one would expect the campaign appeals of minor parties to reflect their chances of achieving a measure of legislative representation. Disproportionality in the translation of votes into legislative seats should therefore lower voter turnout.

### Multipartyism

In contrast, institutional arrangements that allow elections a more decisive role in government formation and that allow for more decisive governments should increase voter turnout. Two such institutional factors are relevant to our purposes: first is the number of political parties and second is unicameralism. As Downs has argued, voters in multiparty systems that produce coalitions face a fundamental problem: they do not directly select the government that will govern them. Instead, they vote for parties that select a government in the legislature, so that "ambiguity and compromise are introduced on a secondary level whenever coalitions are formed" (Downs 1957, 155). This introduces a paradox for multiparty systems: "The type of political system which seems to offer the voter a more definite choice among policies in fact offers him a less definite one. This system may even make it impossible for him to choose a government at all. Instead, it may force him to shift this responsibility onto a legislature over which he has very little control between elections" (p. 156). Downs's argument implies that elections play a less decisive role in government formation within multiparty systems. Where elections are less important, citizens have less incentive to vote. Multipartyism should therefore depress turnout.[1]

### Unicameralism

Beyond the issue of the decisiveness of elections in government formation is the question of the decisiveness of the governments that ensue.[2] Here, I suggest that the degree to which the first legislative body is constrained or checked by other institutions is the relevant quantity. That is, unicameralism is important in producing decisive governments. Where there is no second house (as in New Zealand), governments based on the first house do not have to compete and compromise with another legislative chamber. In contrast, where there is strong bicameralism (as in West Germany and Switzerland), legislation can only be produced by compromise between members of the two houses. This means that elections for the lower house play a less decisive role in the production of legislation where bicameralism is strong. Unicameralism should therefore foster turnout.

The four factors I have introduced are somewhat interrelated. For example, single-member districts favor two-party systems (Duverger 1963), and proportional representation and electoral proportionality are related to each other (Rae 1971a). Indeed, one might argue that the four factors I have discussed simply summarize the distinction between majoritarian and consensual forms of democracy (Lijphart 1984). While there is an element of truth in this argument, it is easily overstated. The interrelations are not strong. Instead, most democracies include mixtures of these characteristics, and there are few pure types (see, e.g., the essays in Bogdanor and Butler 1983). The existence of unicameralism and proportional representation in Israel is a good case in point. The broader issue is that no institutional arrangements are neutral. All involve, in one form or another, a "mobilization of bias" to use Schattschneider's (1960) felicitous term. The task, then, is to evaluate how these biases impinge upon voter turnout.

### Compulsory Voting Laws

Finally, a small number of democracies (Australia, Belgium, and Italy) have laws that mandate voting, as did the Netherlands until 1970. One would expect such "compulsory" voting laws to increase turnout (Gosnell 1930, 184–85; Tingsten 1937). At the same time, the quotation marks are deliberate. The average turnout from 1971 to 1980 in Australia, for example, was 84% (Powell 1986). While this is a high figure, it also indicates that a substantial number of Australian citizens did not vote. I am unaware of any reports either that Australian authorities have shown an inclination or possess the resources to prosecute actively all nonvoters. Similarly, while voting is compulsory in Italy, "the only penalty incurred by an offender is to have his name posted outside the town hall in his commune of residence, and to have his 'certificate of good conduct,' now fallen largely into disuse, stamped 'Did not vote' for five years" (Seton-Watson 1983, 111). Thus, while the presence of mandatory voting laws does provide a disincentive to nonvoting that should increase turnout, there is no reason to anticipate that such laws generate total compliance.

## Data Analysis

My empirical analysis centers on voter turnout in the industrial democracies in the 1970s, and I replicate the results with comparable data for the 1960s. To be considered an industrial democracy, the country had to be continuously democratic since about 1950. This results in a list of 19 states, including the Western European democracies, Israel, Canada, the United States, Australia, New Zealand, and Japan (see Table A-1). My list of countries is the same as Powell's (1986, 38), except that I exclude Spain in view of the fact that Franco did not die until late 1975, and the subsequent transition to democracy was neither smooth nor immediate.

Of the 19 democracies in Table A-1, two stand out as atypical in terms of voter turnout: Switzerland and the United States. In Switzerland, women were not granted the right to vote in national elections until 1971, which means that they were excluded from citizenship until that date. Additionally, the Swiss collective national executive, in effect since 1943, is a unique organizational form that minimizes competition at the federal level. Also, the federal government is not the major provider of collective goods because the cantons are the more important locus of policy-making activity (Rabushka and Shepsle 1972; Steiner 1974). As a result, in Swiss federal elections "there is little incentive for voters to go to the polls, or for the major parties to mobilize them" (Powell 1982, 119). In sum, a series of extraordinary patterns converge in Swiss national politics to minimize voter turnout.[3]

Similarly, electoral law in the United States retains a number of provisions that depress voter turnout. This is the case despite the reforms associated with the Voting Rights Act and its amendments (Wolfinger and Rosenstone 1980), and despite the fact that the U.S. federal government remains a major provider of public goods and a major policy-making arena. The most notable electoral law that is unique to the United States and that has depressed turnout is the widespread 30-day residency requirement for voter registration.[4] The idiosyncratic features of Swiss and U.S. politics are addressed in my analysis below.

### Measures

The dependent variable is the *average voter turnout,* expressed as a percentage of the eligible population. Data were collected for two periods: 1971-80 and 1960-70. Two points need to be made here. First, the eligible population is not restricted to those registered to vote but includes all population members of voting age. Second, with two exceptions, the turnout figures refer to elections for the first (or lower) legislative house. The figures for France and the United States reflect average turnout in presidential elections. Total turnout figures are taken from Mackie and Rose 1982. The eligible voting population was calculated for each country from data reported by the International Labour Office (1977) and by the United Nations (1984). Country values by decade are listed in the Appendix, along with further details on the calculation of voting rates.

To gauge the degree to which there are *nationally competitive districts,* I rely on Powell's figures. Countries with national elections by proportional representation or a national pool for some legislative districts or a simple national presidential vote are assigned a score of four; those with proportional representation in large districts receive a score of three; countries with proportional representation and three to five members per district are scored two; and countries with single-member or winner-take-all districts receive the lowest score of one. Values for this variable on all countries are listed by Powell (1986, 38).

*Electoral disproportionality* is measured using Lijphart's (1984, 160-65) index of disproportionality. This index takes the average vote-seat share deviation of the two largest parties in the years from 1945 to 1980. According to Lijphart's figures, the most proportional system in the period was Denmark, with a score of 0.9. The least proportional was France (Fifth Republic), with a score of 12.3. Country values are listed in Lijphart 1984 (Tbl. 9.1).

*Multipartyism* is gauged with a measure developed by Laakso and Taagepera (1979) of the effective number of political parties in the legislature. This measure constitutes a rearrangement of Rae and Taylor's (1970) index of fractionalization: the higher the score, the greater the distance from a pure two-party system. Data for the 19 countries are from Lijphart (1984, Tbl. 7.3) and again refer to the 1945-80 period.[5] Scores range from 1.9 (United States) to 5.0 (Finland and Switzerland).

*Unicameralism* is measured using the criteria proposed by Lijphart (1984, 212-13), who suggests that two features are of particular importance. First, whether bicameralism is asymmetrical in a manner that favors the lower house or symmetrical (as in Belgium and Italy). The more symmetrical, the stronger the bicameralism. Second, whether bicameralism is "congruent" in the sense of both houses' being very similar in composition, or do the two houses reflect very different sets of interests? The more congruent, the weaker the bicameralism. Employing these criteria, Lijphart assigns the highest score of four for unicameral countries; three for congruent and very asymmetrical bicameralism; two for incongruent and very asymmetrical bicameralism; one for bicameralism; and zero for strong bicameralism. Country scores are made on the basis of Lijphart's empirical classifications in his Tables 6.3 and 6.4, except that Sweden is scored four after 1970 and two before that year because it has been unicameral since 1970 but was bicameral before that (see Lijphart 1984, 213, for the scoring system).

Table 1. Summary Statistics for all Variables

| Variables | Minimum | Maximum | Mean | Standard Deviation |
|---|---|---|---|---|
| Voter turnout, 1970s | 43.3 | 93.5 | 78.36 | 12.23 |
| Voter turnout, 1960s | 25.1 | 93.4 | 77.55 | 15.09 |
| Nationally competitive districts | 1.0 | 4.0 | 2.68 | 1.20 |
| Electoral disproportionality | .9 | 12.3 | 3.67 | 3.01 |
| Multipartyism | 1.9 | 5.0 | 3.28 | 1.06 |
| Unicameralism, 1970s | 0 | 4.0 | 2.11 | 1.59 |
| Unicameralism, 1960s | 0 | 4.0 | 2.00 | 1.53 |
| Compulsory voting, 1970s | 0 | 1.0 | .16 | .37 |
| Compulsory voting, 1960s | 0 | 1.0 | .21 | .42 |
| Number of cases | | | | 19 |

Finally, *compulsory voting* laws are represented by a dummy variable that equals one where such laws are present and zero otherwise. Following Powell (1986, 38), three countries receive a score of one on this variable for the years after 1970: Australia, Belgium, and Italy. For the decade of the 1960s, Holland is also scored one (compulsory voting was abolished in the Netherlands in 1970).

In Table 1, I report the summary statistics for all variables. For the 1970s, turnout ranges from 43% (Switzerland) to 93% (Italy), with a mean of 78.4% and a standard deviation of 12.2. Switzerland and the United States have extreme (low) turnout values.[6] With these two cases removed, the minimum turnout score is 67% (Canada), the mean increases slightly to 81.8%, while the standard deviation is almost halved (to 6.5). In other words, Switzerland and the United States are responsible for a good portion of the observed variance in turnout across the 19 democracies. At the same time, the removal of these two cases has no discernible impact on the summary statistics among the explanatory variables in Table 1. This suggests that the distinctively low turnout rates of Switzerland and the United States are not due to the institutional factors listed in Table 1, since those two countries do not exhibit unusual values for these characteristics. Instead, the low turnout would appear to be a function of other institutional patterns that are unique to these two cases.[7]

With one exception, the remaining summary statistics for the explanatory variables in Table 1 are not particularly sensitive to any unusual country values. The exception is the French score of 12.3 on electoral disproportionality (the next highest score is 8.1 for Canada). Removal of the French score reduces the mean score on this variable from 3.7 to 3.2, and the standard deviation drops from 3.0 to 2.2. I shall return to this issue below.

## Parameter Estimates

Table 2 displays two sets of estimates of the effects of institutional procedures on voter turnout in the 1970s. In the first column, turnout is regressed on the five explanatory variables listed in Table 1. Inspection of the coefficient estimates indicates that each has the expected sign, although the size of many of the coefficients seems excessive. For example, it is hard to believe that mandatory voting laws raise turnout by 22 percentage points on the average. With the exception of electoral disproportionality, all parameter estimates have t-ratios larger than 2.0, which means that they are statistically significant beyond the .05 level (using two-tailed tests). Overall, the model ac-

**Table 2. Regressions of Voter Turnout on Institutional Characteristics for the Industrial Democracies, 1971–80**

| Regressor | Coefficients (t-ratios)<br>Equation Without Swiss and U.S. Dummies | Coefficients (t-ratios)<br>Equation Including Swiss and U.S. Dummies |
|---|---|---|
| Nationally competitive districts | 6.09<br>(3.1) | 3.56<br>(3.7) |
| Electoral disproportionality | - .41<br>(0.6) | - .79<br>(2.5) |
| Multipartyism | -6.18<br>(2.7) | - 2.99<br>(2.5) |
| Unicameralism | 4.69<br>(3.9) | 1.69<br>(2.4) |
| Compulsory voting | 22.18<br>(4.4) | 13.09<br>(4.9) |
| Switzerland dummy | | -29.62<br>(6.4) |
| United States dummy | | -17.62<br>(4.3) |
| Constant | 70.44<br>(8.2) | 78.37<br>(17.7) |
| $R^2$ | 74.8 | .96 |
| F-ratio | 7.72 | 33.21 |
| Number of cases | | 19 |

counts for three-quarters of the variance and has a secure F-ratio.

However, the estimates in the first column of Table 2 are naive in the sense that they take no cognizance of the particularly low turnout rates of Switzerland and the United States. Accordingly, the second column of the table shows estimates of the basic model modified to include a dummy variable for each of these two cases. Comparing the two columns, we see that the addition of these two country-dummy variables has a pronounced impact on each of the coefficient estimates. Four of those estimates are reduced by between 36% and 59% of their original values to more reasonable levels, while the estimate for electoral disproportionality increases by 92%. Additionally, all estimates have the anticipated sign, and each has an acceptable t-ratio. Both country dummies are sharply negative with highly significant t-ratios, and the revised model accounts for 95% of the variance in national rates of turnout.

Comparing the two sets of estimates is instructive. First, it is clear that those in the first column are not robust and that Switzerland and the United States are influential cases. This is evident from the shifting coefficient estimates just discussed. It is also apparent from the t-ratios of 6.4 and 4.3 for the country dummies, which are equal to the studentized residuals (a measure of influence) for those two cases (Bollen and Jackman 1985, 517). Thus, the estimates in the first column of Table 2 do not reflect general patterns.

Second, it is important to understand

that the revised estimates in the second column mean that Switzerland and the United States have been removed from the analysis. Each takes up a complete degree of freedom. Thus, for both cases, the "fitted" turnout values are identical to the actual turnout rates (see Table A-1). Indeed, the same estimates and t-ratios for the five explanatory variables are obtained with these two cases excluded (N = 17).[8] This implies that the estimated effects of the five explanatory variables apply to all of the democracies except for Switzerland and the United States. Consequently, we can draw no conclusions from the estimated model about the effects of, say, unicameralism or multipartyism for these two cases. Given the evidence at hand, explanations of turnout in Switzerland and the United States couched in terms of the unique institutional configurations in each of those countries must remain qualitative.

## Stability of the Estimates

How stable are the estimates in the second column of Table 2? To address this question, I first checked for possible collinearity. In the last section I suggested that the relations among the explanatory variables are not high. The largest zero-order associations (N = 19) are between multipartyism and nationally competitive districts (r = .65), and between multipartyism and electoral disproportionality (r = −.48); with the scores for Switzerland and the United States removed, these correlations are .66 and −.52, respectively. A more rigorous test involves regressing, in turn, each explanatory variable on the remaining explanatory variables. Results of this procedure suggest a pattern similar to that indicated in the zero-order correlations. Thus, the explanatory variables are systematically associated with each other in the ways that students of electoral systems would expect. However, given the size of these associations and given that the principal effect of collinearity is to inflate standard errors, collinearity is not a major problem in the present context.

Along with these checks, I examined the estimates for robustness to evaluate the extent to which any countries might be exerting a disproportionate effect on the estimated parameters. Here, I relied primarily on partial regression plots (see Belsley, Kuh, and Welsch 1980; Bollen and Jackman 1985; and Velleman and Welsch 1981). These plots revealed one potential problem: the partial plot for electoral disproportionality flagged France as possibly problematic largely because the French score on this variable is relatively high. I checked whether France was an influential case in two ways. First, to pull in the high values, I performed a simple rank-order transformation on electoral disproportionality and reestimated the model in Table 2, column 2, substituting the transformed variable for the original. The resulting estimates are very similar to those reported in Table 2 and lead to exactly the same conclusion. Second, I added a country-dummy variable for France to the model in Table 2, column 2. The estimate for this country dummy has a t-ratio of only 0.4.

That France is not an influential case is of particular interest for two reasons. First, this is the only country in the analysis that experienced profound constitutional changes after 1950.[9] Second, France and the United States are the only two countries in the analysis that have an unambiguously presidential form of government (Lijphart 1984, 70-71), and I have therefore—like Powell—calculated turnout scores for France from presidential, rather than legislative, elections. The United States' scores have already been removed from the analysis. Generalizing from only two cases can most charitably be regarded as hazardous. However, because France does not appear to be an influential case, it seems reasonable to conclude that presidential forms of government are not distinctive in terms of electoral turnout. More generally, the diagnostic tests indicate that the results are relatively robust.

As a third test for stability, I examined the partial regression plots for evidence of heteroscedasticity. I also calculated the rank-order correlations between the absolute value of the residuals (ABSRES) from the estimates in Table 2, column 2 and each of the explanatory variables (see Johnston 1972) and regressed ABSRES on all of the explanatory variables. None of these tests provided systematic evidence of heteroscedasticity. I conclude from all of these procedures that the second set of estimates in Table 2 is not seriously affected by collinearity, the presence of influential cases, or heteroscedasticity, and, in this sense, that those estimates are reasonably stable.

## Other Considerations

A brief comparison of my specification with Powell's (1986, Tbl. 2) is in order because he includes three factors that I have not. First, Powell considers the percentage of the population over 34 years of age as an explanatory variable because survey evidence indicates that young citizens have lower rates of turnout. His own results, however, indicate that this variable has no systematic effect (Powell reports a coefficient of .38 and a standard error of .28, which implies a t-ratio of 1.36). Second, Powell controls for the presence of automatic registration laws. But only the United States and France have voluntary registration, and even these two cases are quite distinct from each other, as I have already indicated. I conclude that the critical distinction is between the voluntary registration of the United States (accompanied, as it is, by no other requirements that citizens register) and all other voter registration systems. This distinction is captured already by the country dummy for the United States. Third, Powell includes a measure of the linkage between social groups and political parties. This variable is like a measure of class voting (see Alford 1964), except that it can reflect occupation *or* religion *or* church attendance. Whichever of these shows the highest group-party linkage in a given country is taken as the score for that country. However, this variable is a little difficult to interpret because the measure of group-party linkages refers to different groups in each country and because it does not include all potentially relevant groups (for example, religious voting is counted but language-based voting is not). Nor have I found any clear statistical evidence to warrant its inclusion.[10]

The task that remains is to evaluate the coefficient estimates in the second column of Table 2. I have already noted that all coefficients are of the expected sign. Now we need to evaluate their magnitude. The estimate for type of electoral district is 3.6. Because this variable ranges from 1 to 4, the estimated coefficient indicates a net spread of over 14 percentage points in turnout between countries with single-member districts (like New Zealand and the United Kingdom) and those with national "districts" (such as Israel and the Netherlands). Similar calculations for the remaining explanatory variables indicate the following. Those countries where the vote-seat translation is proportional (e.g., Denmark and Sweden) have a turnout advantage of about 9 points over France, where the vote-seat translation is most disproportional. On the other hand, two-party systems (e.g., New Zealand and the United Kingdom) enjoy a turnout advantage of 12 points over multiparty systems (e.g., Finland and the Netherlands). The smallest net effect is for unicameralism: the presence of unicameral legislatures in countries like Finland and Norway produces a turnout increase of about 8 points over the rate that prevails with strong bicameralism (as in Australia and West Germany). Finally, with all these factors controlled, mandatory voting laws increase turnout by 13 points in the three countries where they apply.

I emphasize that the preceding are net effects. Further, because few countries have a purely majoritarian set of institutional arrangements, these effects offset each other in some cases. For example, the turnout-enhancing impact of nationally competitive elections in the Netherlands is offset by the existence of multipartyism in that country. The turnout-inhibiting effect of multipartyism is similarly compensated by unicameralism in the case of Finland. On the other hand, the relatively low turnout in Canada reflects a set of institutional arrangements (single-member districts, electoral disproportionality, and weak bicameralism) that dampen participation in a more cumulative manner. And the higher turnout in Austria is due to a set of procedures (competitive elections, more electoral proportionality, and only nominal bicameralism) that facilitate voting in an equally cumulative way.

In light of these calculations, which reflect the observed ranges of all the variables in the industrial democracies, the effects of nationally competitive districts and of mandatory voting laws appear to be the strongest. But differences in the magnitudes of the coefficients are not large, and the effects of the remaining three institutional factors remain pronounced.

## Replication

The evidence adduced to this point indicates that political institutions have a major impact on rates of voter turnout. Yet it remains the case that the model is based on a comparatively small number of cases and that it applies only to elections that took place in the 1970s. Are the results unique to that decade?

To address this question, I gathered data on voter turnout in the previous decade (1960-1970) for the same 19 democracies. Sources and estimation procedures for the turnout data are the same as before and are listed in the Appendix, along with country values. Comparing average turnout rates for 1960-70 with those for 1971-80 indicates that country-turnout levels were relatively stable across both decades (for similar observations, see Crewe 1981; Powell 1980). But some systematic shifts are also evident. For example, turnout dropped eight percentage points in the Netherlands in the 1970s, while it rose by a smaller amount in Sweden in the same period.

The explanatory variables, of course, refer to long-term constitutional arrangements that have varied little since 1950. Indeed, the major constitutional transformation since that date came with the formation of the Fifth French Republic in 1958. In view of this, the explanatory variables for the analysis of turnout in the 1960s are identical to those employed for the 1970s analysis, with two exceptions. First, the Swedish reform of 1970 abolished the upper legislative house so that the Riksdag has been unicameral since that date. Second, mandatory voting was abolished in the Netherlands in the same year. As indicated earlier, the appropriate variables are modified for the 1960s analysis to reflect these facts.

In Table 3, I report the estimated effects of political institutions on average voter turnout, 1960-70. Because it includes the country dummy variables for Switzerland and the United States, this set of estimates can be compared directly to the figures in the second column of Table 2. The major difference between the two decades occurs with the country-dummy variables. For the 1960s, the adjustment for Switzerland is −.43, which is much larger than the corresponding adjustment for the 1970s and reflects the disenfranchisement of Swiss women before 1971. In contrast, the

Table 3. Regressions of Voter Turnout on Institutional Characteristics for the Industrial Democracies, 1960–70

| Regressor | Coefficients (t-ratios) |
|---|---|
| Nationally competitive districts | 3.13<br>(2.8) |
| Electoral disproportionality | - 1.10<br>(3.2) |
| Multipartyism | - 4.06<br>(2.7) |
| Unicameralism | 2.69<br>(2.9) |
| Compulsory voting | 15.03<br>(4.8) |
| Switzerland dummy | -43.03<br>(7.3) |
| United States dummy | - 8.18<br>(1.8) |
| Constant | 80.66<br>(17.8) |
| $R^2$ | .97 |
| F-ratio | 43.69 |
| Number of cases | 19 |

adjustment for the United States is −8 for the 1960s, which is considerably *smaller* than the 1970s adjustment of −18. As is well known, voter turnout declined considerably in the United States from 1960 to 1980 (for an analysis of this decline, see Abramson and Aldrich 1982).[11]

All of the remaining coefficient estimates have the same sign, are statistically significant, and are of similar magnitude across the two decades. As a result, the 1960s analysis simply reinforces the inferences drawn first for the 1970s. At one level, this reflects the fact that constitutional arrangements have largely been in place throughout the period. But the replication also indicates that when political institutions are altered, voter turnout is affected. Turnout fell after the abolition of mandatory voting in the Netherlands, and the introduction of unicameralism in Sweden was followed by an increase in voter turnout.

## Implications

The preceding analyses indicate that national political institutions and electoral laws provide an important incentive structure for voter turnout. The model I have presented accounts rather well for variations in turnout across 17 of the 19 democracies. And the distinctive turnout rates of the two remaining countries are most plausibly linked to their unique political institutions and laws.[12] What are the general implications of this conclusion?

First, in conjunction with Powell's (1986) analysis, my results challenge the idea that national differences in voter turnout reflect national differences of political culture. Powell's cross-national survey analyses suggest that U.S. citizens have the edge in participatory attitude structures (see also Almond and Verba 1963; Wolfinger, Glass, and Squire 1985). However, differences in participatory norms are not, in turn, systematically linked to turnout rates. For example, as Crewe noted six years ago,

> The best known (and still most useful) comparative study of [subjective] orientations, *The Civic Culture*, found that interest in politics, attention to political affairs in the media, feelings of civic duty and of individual political efficacy, and trust in political as opposed to other solutions to individual and communal problems, were consistently highest in the United States, followed by Britain, then Germany, and finally Italy—exactly the *reverse* of their rank order for postwar turnout! (1981, 239; italics added)

In short, even with the United States excluded, national differences in norms are unrelated to variations in turnout. More recent cross-national survey data are no kinder to a political-culture explanation (Powell 1986; Wolfinger, Glass, and Squire 1985). Coupled with my analyses, these data undermine the plausibility of any culture-turnout linkage.[13]

Consider the cases of Australia and New Zealand, two countries with similar political histories and cultures. This shared background is reflected in many institutional arrangements, including single-member districts, electoral disproportionality, and a near two-party system. Moreover, turnout has persistently been only slightly higher in Australia (84.5% versus 83.5% in the 1970s, and 84% versus 82% in the 1960s). Some might conclude that this similarity in turnout rates reflects shared cultural patterns. It is more fruitful, however, to note that along with all of their shared characteristics, there are two major institutional differences, the effects of which almost offset each other. Australia has mandatory voting and strong bicameralism, while voting is not compulsory in New Zealand, which has been unicameral since the abolition of the Legislative Council in 1950. Given these two differences and my estimates, we would expect marginally higher turnout in Australia because both mandatory voting and unicameralism increase turnout, but the former effect is more pronounced.

In contrast, Canada, the other old British dominion, shares many cultural features with both of these countries, along with single-member districts and a similar party system. However, the Canadian turnout figure is only 67%. Unlike Australia, Canada lacks mandatory voting laws, and unlike New Zealand, it lacks strong unicameralism. Both of these factors, along with an even less proportional electoral system, hold Canadian turnout well below the observed rates in Australia and New Zealand.

One other example is useful. Norway and Sweden have many historical, cultural, and institutional commonalities. For example, they have similar forms of electoral representation, the same effective number of political parties, and both are unicameral. However, Lijphart's figures indicate a higher degree of disproportionality in the translation of votes into legislative seats in Norway than in Sweden. According to my model, this accounts for the lower average rate of turnout in Norway (81% versus 87% in Sweden in the 1970s). Note, too, that the abolition of the Swedish upper house in 1970 resulted in an increase in average voter turnout of four percentage points in the subsequent decade.

It is difficult to see how differences like these can be explained by varying political cultures.[14] Along with the analyses by Powell (1980, 1986) and Wolfinger, Glass, and Squire (1985), my results suggest that it is much more profitable to account for such differences in terms of the kinds of incentives to voting generated by varying national political institutions and laws.[15] I emphasize again, however, that the critical distinction is not between majoritarian and consensual constitutional arrangements. Instead, the evidence indicates that both types of arrangements have some features that increase the incentive to vote and other features that inhibit that incentive. It appears that the overall constitutional arrangement is less important than its specific components in influencing voter turnout.

Second, my results have implications for the way in which we interpret national differences in rates of voter turnout in the industrial democracies. Some studies have included voter turnout as one component of a measure of political democracy (e.g., Jackman 1973). Others have questioned this practice on the grounds that the meaning of national variations in turnout is ambiguous (e.g., Bollen 1980). Specifically, it has been suggested that variations in voter turnout may reflect apathy or alienation and that there is no clear way to choose between the two (e.g., Lipset 1960).

Some go much further to suggest that low turnout reflects satisfaction with the political status quo and is therefore a good thing. For example, just over 50 years ago Wilson wrote, "In a society in which only 50 percent of the electorate participates it is clear that politics does satisfy in a way the desire of the mass of the individuals in the state. As the percentage of participation rises above, let us say, ninety percent, it is apparent that the tensions of political struggle are stretching to the breaking point the will toward the constitutional" (1935, 76. See also Berelson, Lazarsfeld, and McPhee 1954, chap. 11; Wilson, 1930). To be fair, Wilson was trying to explain events in Europe in the 1930s. But it is interesting to note that, when applied to 1970s turnout figures, his argument suggests that Swiss and U.S. citizens should be distinctively satisfied, while Italy and Sweden should be on the verge of collapse. It is of equal interest that at least one of Wilson's contemporaries concluded that electoral turnout in Europe was best explained by the structure of political competition and electoral laws (Gosnell 1930).

Extending Wilson's argument a little, Morris-Jones asserted that the notion that high turnout is desirable belongs properly to "the totalitarian camp" (1954, 25). Indeed, apathy is the handmaiden of democracy:[16]

> The presence of an apathetic part of the electorate . . . may also have a beneficial effect on the tone of political life itself. For this group is a more or less vivid reminder of the proper limitations of politics, a more or less effective counterforce to the fanatics who constitute the real danger to liberal democracy. A State which has cured apathy is likely to be a State in which too many people have fallen into the error of believing in the efficiency of political solutions for the problems of ordinary lives. (1954, 37)

After examining popular support for democratic principles, Prothro and Grigg concluded that apathy may be "functional" for democracy: "fortunately for the democratic system, those with the most undemocratic principles are also those who are least likely to act" (1960, 294). Lipset's analysis of working-class authoritarianism (1960, chap. 4) also implies that high turnout may often be undesirable because it involves political participation by those with antidemocratic tendencies.

Coming from a slightly different tack, Eckstein argued for the importance of "balanced disparities" in preserving stable democracies: "Democratic governments require a healthy degree of authoritarianism not only for the sake of congruence between government and other aspects of society, but for the even simpler reason that a representative government must govern as well as represent—must satisfy two values which, on the evidence, are not easily reconcilable" (1966, 265-66). According to this line of thought, high levels of participation may not be desirable because participation conflicts with other valued processes. Thus, citizen involvement is a phenomenon that is perhaps best enjoyed in moderation. Almond and Verba state the case well:

> The most striking characteristic of the civic culture . . . is its mixed quality. It is a mixture in the first place of parochial, subject, and citizen orientations. The orientation of the parochial to primary relationships, the passive political orientation of the subject, the activity of the citizen, all merge within the civic culture. The result is a set of political orientations that are managed or balanced. There is political activity, but not so much as to destroy governmental authority; there is involvement and commitment, but they are moderated; there is political cleavage, but it is held in check (1963, 492-93).

More recently, Huntington has argued that postindustrial societies may, in fact, face a problem of overparticipation: "Widespread education tends to produce *too much interest and participation* which leads in turn to political stalemate. Innovation is easier when substantial portions of the population are indifferent" (1974, 177; italics added). In other words, when participation levels become excessive, a crisis of governability may be induced.

The results of my paper suggest that the significance of national differences in rates of voter turnout is less ambiguous than some have suggested, and, further, that high rates of voter participation are not pernicious. Specifically, the odds that citizens will vote vary with the structural incentives that they confront. Moreover, if cross-national variations in turnout are primarily driven by differing institutional arrangements, then high rates of voter turnout cannot be taken in themselves as evidence of subjective participatory norms (as is done, e.g., in Putnam et al. 1983).

That voting turnout is low when laws prevent or actively discourage large groups of people from voting is a truism—contemporary South Africa, Switzerland until 1971, or the U.S. southern politics of yesteryear (Key 1949) are the blatant cases in the post-World War II world. My analysis indicates that even when we place such extreme cases to one side, legal and institutional variations retain a critical role. Where institutions provide citizens with incentives to vote, more people actually participate; where institutions generate disincentives to vote, turnout suffers. Thus, the meaning of national differences in voter turnout is rather clear: turnout figures offer one gauge of participatory political democracy.

## Appendix

In Table A-1, I report voter turnout for the 19 industrial democracies.

Figures in the first and third columns are average turnout levels for 1971-80 and 1960-70, respectively, and entries in parentheses indicate the number of elections on which the averages are based. Turnout levels reflect total turnout divided by the size of the population of voting age. Figures in the second and fourth columns are the fitted turnout levels for the two decades and are based on the estimates in Table 2, column 2 and Table 3, respectively.

Total turnout figures are from Mackie and Rose 1982 and, with two exceptions, are for national elections to the lower legislative house. For France and the United States, total turnout figures are for presidential elections. In each decade, France is counted as having four elections because I have averaged turnout from first- and second-round presidential elections. In addition, because there was only one election from 1971 to 1980 (in 1974), I have included turnout from the 1981 election for France only.

Estimates of the size of the population of voting age are based on figures published by the International Labour Office (1977) for 1960 and 1970, and by the United Nations (1984) for the early 1980s. Since 1960, and especially in the 1970s, most countries lowered the voting age at least once, and often more. The size of the population of voting age reflects these

Table A-1. Countries and Turnout Values for the 1960s and 1970s

| Country | Turnout 1971–80 | Fitted Turnout 1971–80 | Turnout 1960–70 | Fitted Turnout 1960–70 |
|---|---|---|---|---|
| Australia | 84.48(5) | 83.13 | 83.91(4) | 82.51 |
| Austria | 87.76(3) | 85.97 | 90.10(3) | 87.00 |
| Belgium | 87.83(4) | 91.03 | 86.87(3) | 90.34 |
| Canada | 67.15(4) | 71.76 | 70.75(4) | 70.51 |
| Denmark | 86.77(5) | 85.81 | 86.53(4) | 85.51 |
| Finland | 82.18(4) | 79.60 | 84.12(3) | 78.77 |
| France | 76.84(4) | 76.45 | 70.80(4) | 71.62 |
| West Germany | 84.22(3) | 83.19 | 82.74(3) | 80.32 |
| Ireland | 76.56(2) | 80.30 | 73.65(3) | 80.99 |
| Israel | 82.15(2) | 84.45 | 81.79(3) | 83.67 |
| Italy | 93.47(3) | 91.63 | 93.42(2) | 91.15 |
| Japan | 72.91(4) | 74.60 | 70.02(4) | 72.40 |
| Netherlands | 81.54(3) | 78.78 | 89.60(2) | 89.81 |
| New Zealand | 83.61(3) | 77.75 | 81.82(4) | 79.50 |
| Norway | 81.32(2) | 83.80 | 81.65(3) | 84.42 |
| Sweden | 87.46(3) | 88.86 | 83.96(4) | 84.26 |
| Switzerland | 43.29(3) | 43.29 | 25.08(2) | 25.08 |
| United Kingdom | 74.99(3) | 74.15 | 74.84(3) | 73.82 |
| United States | 54.22(3) | 54.22 | 61.73(3) | 61.73 |

changes so that, for each election, total turnout is divided by the size of the population eligible to vote on age grounds *in that election.* For example, in Denmark the voting age was reduced from 21 to 20 in 1972, and then lowered to 18 in 1978, which affected the size of the Danish population of voting age in the 1970s. Information on voting-age requirements is from Mackie and Rose 1982, supplemented by *Keesing's Contemporary Archives* and *Facts on File.* Further restrictions on voter eligibility not based on age (most notably in Switzerland and the United States) are not reflected in the denominator.

Two further points are relevant. First, the International Labour Office/United Nations population-size tables by age report population by age *groups,* and in the age range relevant here, those groups consist of five-year aggregations: 15-19 and 20-24. I have assumed that each five-year age group consists of five equally sized annual age groups. Thus, for example, to obtain the size of a population aged 21 or more, I have subtracted from the total national population all those aged 0-19 plus one-fifth of those aged 20-24. Second, the tables do not list annual figures. To determine the size of the voting age population in a given election year, I have interpolated from the available data. My interpolations are based on estimates of annual population-growth rates assuming logarithmic patterns of growth (for the rationale and procedures used, see Barclay 1958; Jackman 1980).

The country-turnout values for the 1970s listed above are very similar to those reported by Powell (1986, app. 1). Minor discrepancies are probably due to rounding differences and slight differences in the estimates of the size of the voting-age population. A regression of my figures for 1971-80 on Powell's figures for the same period (N = 19) produces an intercept that is indistinguishable from zero (with a t-ratio of 0.15), a coefficient of 1.001, and an $R^2$ of .994. In view of this, it is not surprising that when the models in

Table 2 are reestimated substituting Powell's turnout figures for my own, the estimates obtained are almost identical to those reported in this paper.

## Notes

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1. Some may find this expectation counterintuitive. For example, Mueller and Murrell argue instead that "to the extent that greater voter participation is a result of larger turnout by the enfranchised poor, rather than differences in enfranchisement across countries, one might expect the poor to participate in greater numbers the greater the competition for their support among existing parties. In turn, more party competition can be expected the greater the number of viable political parties" (1986, 139). The last sentence is a nonsequitur. Although the *nature* of political competition may change as the number of parties increases beyond two, there is no reason to believe that more parties should generate more competition (of course, one-party systems remain monopolies that are beyond the scope of this paper).

2. I am hardly the first to address the role of electoral decisiveness. Most attention, however, has centered on the role of decisiveness in the process of government formation rather than on the decisiveness of the governments that ensue (e.g., Dahl 1966; Rae 1971b; Strom 1984). And electoral decisiveness is seldom seen as part of the institutionally induced incentive structure that shapes the volume of electoral participation.

3. Gosnell also concluded that turnout in Switzerland was lower than in the rest of Europe. His explanation: "In Switzerland, there are many more elections than in the other countries studied. The Swiss have shown the same indifference toward the election of minor officers that is manifested in the United States" (1930, 186-87).

4. As Powell (1986, 21) notes, France is the only other country with voluntary voter registration, but French citizens are required to register in their community and to obtain identification cards, which encourages voter registration.

5. In the case of France, Lijphart treats the Fourth and Fifth Republics separately. The measures of electoral disproportionality and multipartyism employed here refer to the Fifth Republic.

6. The figures in Table A-1 probably understate Swiss voter turnout somewhat given the relatively high number of alien residents coupled with the stringent requirements for citizenship in Switzerland. Powell judges that this accounts for much, but not all, of the difference between Switzerland and the United States in voter-turnout rates during the 1970s (see Powell 1986, app. 3 for further discussion).

7. For the 1960s, the minimum voter-turnout rate is an even lower 25% for Switzerland, reflecting the disenfranchisement of women.

8. In either case, there are 11 degrees of freedom. The only differences between the two sets of estimates occur with the summary statistics. This is due to the fact that excluding Switzerland and the United States from the regression analysis reduces the total sum of squares in voter turnout considerably. With these cases excluded ($N = 17$), the $R^2$ is .820 (F-ratio, 9.99). That the $R^2$ is higher with these two cases included (see Table 2, col. 2) also reflects that the two country-dummy variables "fit" the observed Swiss and U.S. turnout scores exactly.

9. This is not to denigrate the significance of the Swiss disenfranchisement of women before 1971. However, as I have already noted, Switzerland has been removed from the statistical analysis.

10. More specifically, the group-party linkage variable has no systematic effect on turnout net of the other variables listed in Table 2.

11. The only institutional changes in the United States since 1960 have been those involved with voting-rights legislation. Because this legislation was designed to increase turnout, the decline in U.S. voting rates since 1960 obviously cannot be explained in terms of the kinds of institutional factors I have considered in this paper.

12. While the model has been estimated with Switzerland and the United States excluded, it is interesting to note that it does imply comparatively low turnout levels in both of these cases. Of the institutional factors I have considered, Switzerland and the United States each have only one turnout-enhancing feature: in the former, a proportional electoral system, and in the latter, a two-party system. However, as the estimates for the country-dummy variables show, turnout rates in these two countries are *much* lower than my model ($N = 17$) implies, which I attribute to their distinctive turnout-inhibiting characteristics, even after 1970.

13. Matters do not improve if we emphasize the importance of "political tradition" in lieu of political culture (Bogdanor 1983). Indeed, my results directly challenge Bogdanor's conclusion that "the comparative study of electoral systems and party systems is likely to be of more use in shedding light on what is unique and particular than in yielding generalizations which do justice to the historical experience of different countries" (1983, 261).

14. Nor can the turnout differences across the industrial democracies be accounted for in terms of a Lerner-style argument that emphasizes the role of literacy and the mass media in generating political

participation (Lerner 1958, chap. 2). All of the states in this study have highly literate populations and well-developed mass-media infrastructures (see, e.g., the figures in World Bank 1983).

15. Skeptics might counter that these national political institutions and laws are themselves a function of political culture. However, if we take the data on national differences in citizens' political orientations described by Powell (1986) and Wolfinger, Glass, and Squire (1985) as identifying national political cultures, then differences in political cultures are not associated with the institutional variations I have addressed in this paper. If political culture is defined in terms much broader than this, it becomes a very blunt analytical instrument.

16. A phrase I first heard uttered by Paul Peretz in an exchange with William F. Buckley, Jr., at Curious Cove, New Zealand, January 1967.

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# INEQUALITY AND INSURGENCY

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*Maldistribution of land in agrarian societies is commonly thought to be an important precondition of mass political violence and revolution. Others argue that because of the difficulty of mobilizing rural populations for political protest, land maldistribution is irrelevant except as part of an inegalitarian distribution of income nationwide. These rival inequality hypotheses have significant implications with respect to the kinds of reforms likely to reduce the potential for insurgency in a society. They are tested using the most comprehensive cross-national compilation of data currently available on land inequality, landlessness, and income inequality. Support is found for the argument that attributes the greater causal import to income inequality. Moreover, the effect of income inequality on political violence is found to hold in the context of a causal model that takes into account the repressiveness of the regime, governmental acts of coercion, intensity of separatism, and level of economic development.*

Many students of domestic political conflict consider inequality in the distribution of land and/or lack of land ownership (landlessness) to be among the more fundamental economic preconditions of insurgency and revolution (e.g., Huntington 1968; Midlarsky 1981, 1982; Midlarsky and Roberts 1985; Paige 1975; Prosterman 1976; Prosterman and Riedinger 1982; Russett 1964; Tanter and Midlarsky 1967). Huntington (1968, 375), whose writing on the subject has been particularly influential, advanced a strong version of the land maldistribution hypothesis as follows: "Where the conditions of land-ownership are equitable and provide a viable living for the peasant, revolution is unlikely. Where they are inequitable and where the peasant lives in poverty and suffering, revolution is likely, if not inevitable, unless the government takes prompt measures to remedy these conditions." However, because mass revolutions are rare events, it is more plausible to relax the postulate that revolution is an inevitable consequence of land maldistribution and to restate the hypothesis: the greater the maldistribution of land, the greater the probability of mass-based political insurgency and, consequently, the greater the *vulnerability* of a country to revolution from below. This weaker, necessary-but-not-sufficient version of the land-maldistribution-leads-to-revolution hypothesis directs attention to the relationship between land distribution and mass political violence.

The land maldistribution hypothesis is based on the assumption that discontent resulting from a highly concentrated distribution of land and/or lack of land ownership (landlessness) in agrarian societies is an important direct cause of mass political violence. Advocates of what has come to be called the "resource mobilization" approach to the explanation of collective protest and violence (e.g., Gamson 1975; Oberschall 1973; Tilly 1978) reject such discontent hypotheses for the reason that inequality and discontent are more or less always present in virtually all societies and that consequently the most direct and influential explanatory factor must not be discontent per se but rather the *organization* of discontent. Thus Skocpol (1979, 112–57), who is skeptical of discontent theories of revolution, argues that the peasant revolts that were a crucial insurrectionary ingredient in the French, Russian, and Chinese revolutions occurred not because of the maldistribution of landholdings but rather because communities of French, Russian, and Chinese peasants had sufficient autonomy from local landlords to enable them to mobilize collectively. By contrast, Midlarsky (1982, 15–20), a proponent of discontent theory, explains the peasant revolts in each of these cases by the fact that rapid population growth severely exacerbated land inequality until a level of deprivation was reached that no longer could be tolerated.

Two contemporary cases cited by Midlarsky and Roberts (1985) in support of the land maldistribution hypothesis are El Salvador and Nicaragua.[1] Compared with other middle-income developing countries, population growth in El Salvador and Nicaragua was above average during the 1960s and 1970s (see World Bank 1981, tbl. 17). Maldistribution of land also was a serious problem, as the Gini coefficient of land concentration was .80 for Nicaragua and .81 for El Salvador (values well above the global mean of .60) and agricultural households without land (i.e., tenants, sharecroppers, and agricultural laborers) amounted to 40% of the total labor force in El Salvador circa 1970, which was the highest level of landlessness in the world at that time (data are not available for Nicaragua).[2] Each country subsequently experienced a relatively high rate of mass political violence, which in the Nicaraguan case culminated in revolution.

But the seemingly obvious conclusion that land maldistribution must have been a primary cause of political violence in El Salvador and Nicaragua ignores the fact that, during the same period of time, two other Central American states, Costa Rica and Panama, remained quite peaceful despite the presence of exactly the same preconditions supposed to have caused the insurgency in El Salvador and Nicaragua. Costa Rica and Panama experienced above-average population growth (in fact, Costa Rica's 3.4% annual population-growth rate during 1960–70 not only exceeded the 2.9% rate registered by El Salvador and Nicaragua but was also among the highest in the entire world); land was concentrated in the hands of the few to about the same degree in Costa Rica (the Gini coefficient was .82) and Panama (Gini coefficient of .78) as in El Salvador and Nicaragua; and the amount of landlessness in Costa Rica (24%) and in Panama (36.2%) ranked ninth and third highest in the world, respectively. Nevertheless, during 1970–77 Panama registered only a single death from political violence, and there were no instances of deadly political violence in Costa Rica (see Taylor and Jodice 1983, vol. 2, tbl. 2.7).

Comparison of Costa Rica and Panama with El Salvador and Nicaragua thus raises the issue of the general validity of the land maldistribution hypothesis: Are Costa Rica and Panama merely exceptions to the rule, or is maldistribution of land in reality a minor or even irrelevant factor in the process that generates insurgency and revolution? That question is significant not only because inequality is frequently assumed in academic writing to be an important determinant of political instability; it also has profound policy implications because land reform has traditionally been a cornerstone of U.S.

efforts to promote political stability in developing countries.

## Inequality, Resource Mobilization, and the Structure of the State

We argue that theories emphasizing land maldistribution as a fundamental precondition of insurgency and revolution are misspecified. They attribute direct causal significance to an inequality variable that plays only a relatively small, indirect part in the generation of mass political violence. We hypothesize that the more important direct cause of variation in rates of political violence cross-nationally is inequality in the distribution of income rather than maldistribution of land. This hypothesis is predicated on the following assumptions:

1. Inequality in the contemporary world generates discontent;
2. Although inequality is present to some degree in all societies, some societies are significantly more inegalitarian than others;
3. Inequality in the distribution of land and inequality in the distribution of income are not *necessarily* tightly connected; in particular, they are sufficiently independent of each other that an effect of one on a response variable such as the rate of political violence does not necessarily imply that the other will have a similar effect;
4. Given the existence of inequality-based discontent, it is more difficult to mobilize peasant communities than urban populations for political protest; peasants normally become the foot soldiers of insurgent movements only if they are effectively organized by a "vanguard" of urban professional revolutionaries.

From these assumptions we derive the following postulates:

1. A high level of income inequality nationwide significantly raises the probability that at least some dissident groups will be able to organize for aggressive collective action. This is because, first, the pool of discontented persons from which members can be drawn will include the more easily mobilized urban areas; and, second, it may be possible for urban revolutionaries to establish cross-cutting alliances with groups in the countryside
2. A high level of agrarian inequality does not necessarily raise the probability that dissident groups will be able to organize for aggressive collective action; this is because the pool of discontented persons from which members can be drawn may be restricted to the countryside, which is difficult to mobilize; consequently, we predict that if income inequality is relatively low, the rate of political violence will tend to be relatively low, even if agrarian inequality is relatively high; wherreas if income inequality is relatively high, the rate of political violence will tend to be relatively high, even if agrarian inequality is relatively low

Our inequality hypothesis, which is based on an integration of discontent (or relative deprivation) arguments (e.g., Gurr 1970) with the resource mobilization approach, can be illustrated by the cases of Costa Rica and Venezuela, where egalitarian redistribution of income occurred despite persisting high agrarian inequality; and the case of Iran, where income inequality worsened, especially in urban areas, despite an egalitarian land reform.

Costa Rica circa 1960 had a relatively inegalitarian distribution of land (the 1963 Gini coefficient was .78) and an extremely inegalitarian distribution of income (the richest 20% of families received 61% of total personal income in 1961). During the decade of the 1960s the distribution of land in Costa Rica became slightly more concentrated (the 1973 Gini coefficient was .82). The distribution of income, however, was substantially altered in an egalitarian direction by democratically elected reformist administrations who pursued welfare-state policies similar to those of European social democratic governments. By 1970 the share of national income accruing to the richest quintile of Costa Rican households had been reduced to 50%.[3] As mentioned above, violent conflict was absent from Costa Rican politics during the 1970s.

Venezuela was a similarly inegalitarian society circa 1960, when a democratic regime was inaugurated. The 1956 Gini index of land concentration was .91—the second highest in the world next to Peru—and the richest quintile of Venezuelan households received 59% of total personal income in 1962. During the 1960s the distribution of land in Venezuela remained highly concentrated (the 1971 Gini coefficient was .91), but the distribution of income became more egalitarian—although not as dramatically so as in Costa Rica—due to a combination of reformist administrations and an expanding petroleum-based economic pie (by 1970 the income share of the richest quintile of households had been reduced to 54%). Deaths from political violence in Venezuela registered a sharp decline over this period (according to Taylor and Jodice 1983, vol. 2, tbl. 2.7, they amounted to 1,392 during the years 1958–62; 155 during 1963–67; 53 during 1968–72; and 9 during 1973–77).

In the early 1960s, the shah of Iran, under pressure from the Kennedy administration, launched his so-called white revolution (it was to be without bloodshed), which accelerated a land reform begun hesitantly in the 1950s. Initially, about 70% of the arable land in Iran was owned by a very small number of landowners. By 1967, according to official sources, land had been redistributed to about 520 thousand families; 3,238 landlords had sold land to 46 thousand families; 153 thousand tenants had received a percentage distribution of land; and 203,049 landlords had written tenancy agreements with over a million peasant families. Thus a relatively large proportion of Iran's rural population benefitted in some way from the land reform, and approximately 15% of peasant families actually became landowners.[4] Although data on land concentration are not available for the end of this period, already by 1960 the Gini coefficient was .63, an intermediate value.

The shah's land reform has been criticized for being superficial in a variety of respects—for instance, the percentage distribution of land received by the 153 thousand former tenants entailed parcels that were below the minimum needed for family subsistence; there was no bureaucracy to ensure that tenancy agreements would be respected; former landowners often were able to keep the best land for themselves; and there was no follow-up program to provide new farmers with assistance in obtaining seed, water, and fertilizer. Nevertheless, there can be no doubt that its massive scope did contribute to some extent to reduction of agrarian inequality in Iran. At the same time, however, the shah's program of "forced modernization" appears to have caused an increased inequality of income among urban households. Data collected by Jabbari (1981) from family expenditure surveys of the Central Bank of Iran and the Iran Statistical Center can be used as a proxy for income. Gini coefficients calculated from these expenditure data by Jabbari (1981, 174) show that while inequality of rural income decreased in Iran during the 1960s and early 1970s, the increasing inequality in urban income offset the rural trend and resulted in an overall trend of steadily increasing inequality in the nationwide distribution of income. Thus the shah's land reforms were overshadowed by macroeconomic policies that produced an inegalitarian upward redistribution of income, which presumably alienated the growing urban middle class. Political violence began to increase in the mid-1970s, culminating in the revolution of 1979.

Of course, income inequality is not the only cause of mass political violence. In Panama, for example, income was distributed very unequally circa 1970, as the richest 20% of households earned 62% of total national income. But by the early 1970s, General Omar Torrijos Herrera, who had led a successful coup d'etat by officers of the national guard in 1968, had crushed all opposition, established firm censorship of the media, and taken control of the judiciary. Ratings of political rights and civil liberties in Panama during the mid-1970s on a scale of one to seven (most free to least free) averaged 6.5.[5] During this period (1973–77) the Torrijos regime in Panama was the most repressive in the Western Hemisphere next to Cuba, where the rating of political rights and civil liberties averaged 6.9. Inequality-induced discontent presumably existed in Panama, and it probably was relatively widespread, but there was little or no opportunity to organize it.

By contrast, Panama's next-door neighbor to the northwest, Costa Rica, enjoyed the distinction in the mid-1970s of being the oldest democracy in Latin America. Since 1949 Costa Rica had held regularly scheduled free and fair elections, the media were uncensored, unions were free to organize, the judiciary was independent of the executive and legislative branches of government, and citizens were not subject to arbitrary arrest. Costa Rica's political and civil rights ratings averaged a maximum score of 1.0 during 1973–77.

The "open" and "closed" political systems of Costa Rica and Panama exemplify polar extremes of regime repressiveness. Differences in regime structure are relevant to the explanation of cross-national variation in mass political violence because they can be assumed to affect three important variables emphasized in some versions of resource mobilization theory (e.g., McAdam 1982): (1) the extent to which dissident groups are able to develop strong organizations, (2) their belief in the likelihood of success of collective action, and (3) the range of political opportunities available to them for achieving their goals.

In the context of an extremely repressive regime, dissident groups are severely restricted in their ability to organize; their belief in the likelihood of success of collective action will probably be low; and opportunities to engage in collective action of any kind will be quite limited. Consequently, under the condition of a high level of regime repressiveness, rational actors most likely will attach a relatively low utility to violent collective action, and the rate of mass political violence therefore should be relatively low.

In the context of a nonrepressive or "democratic" regime, dissident groups will not face significant restrictions on their ability to organize for collective action, and their belief in the likelihood of achieving at least some success from collective action will probably be relatively high. Moreover, a democratic regime structure will afford a variety of opportunities for dissident groups to participate legally and peacefully in the political process. Because the costs of peaceful collective action will be lower than those of violent collective action and because the likelihood of success of peaceful collective action will be reasonably high, rational actors under the condition of a nonrepressive regime structure presumably will usually attach a much higher utility to peaceful as opposed to violent collective action, and, therefore, the rate of mass political violence here too should be relatively low.

In the context of a semirepressive

regime, it is possible for dissident groups to develop relatively strong organizations. However, opportunities to engage in nonviolent forms of collective action that effectively exert influence on the political process are limited. Semirepressive regimes allow only for, in Green's (1984, 154) apt terminology, *"pseudo-participation* . . . an elaborate charade of the participatory process." Polities with pseudoparticipation typically have elections that are not free and fair, legislatures that are little more than debating societies, and a judiciary that is not independent of the will of the executive; the media are subject to censorship at the whim of the executive; and citizens are subject to arbitrary arrest and detention by security forces, which are under the exclusive control of the executive. In short, semirepressive regimes erect a facade of participatory institutions but do not permit popular input to significantly influence governmental output. Because opportunities for genuine participation are restricted, many politically activated citizens may come to perceive civil disobedience and violence as being more efficacious than legal means of pseudoparticipation; and since the expected costs of insurgency may not be perceived to be prohibitive, rational actors may well attach a relatively high utility to aggressive political behavior. Therefore, it is plausible to expect that the rate of mass political violence cross-nationally will be highest under semirepressive authoritarian regimes.

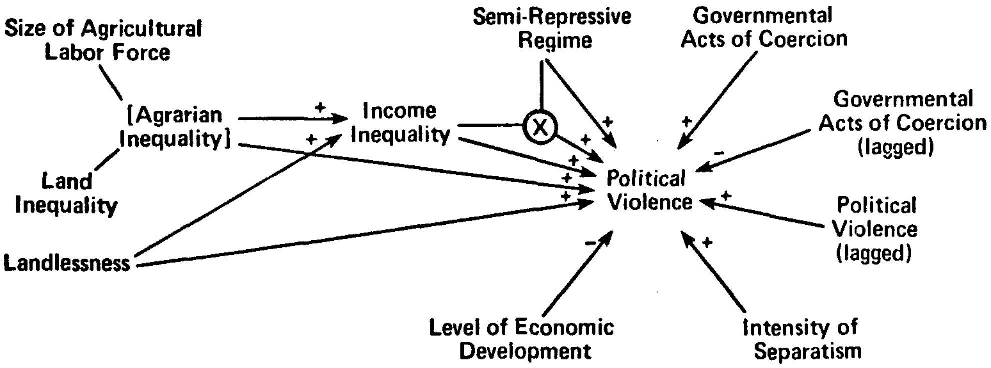
The analysis of the causes of the Iranian revolution by Green (1982, 1984) documents in detail how the shah vacillated between fully restricting mass participation and allowing pseudoparticipation and concludes that "the effects of such tactics served to increase popular hostility among those socially mobilized Iranians eager to have a measure of influence over the manner in which their society was ruled" (Green, 1984, 155). Green's case study description is corroborated by global comparative measures of regime repressiveness, which show that Iran in the late 1950s was classified as having a "semi-competitive" regime (Coleman 1960); was scored for 1960 and 1965 as intermediate (34.9 and 45.0, respectively) on a 0–100 scale of extent of political democracy (Bollen 1980); was ranked circa 1969 at an intermediate level on a scale of opportunity for political opposition (Dahl 1971); received a mean rating of 5.7 on political and civil rights for 1973–77; and had shifted in 1978 to a mean rating of 5.0 on political and civil rights. Thus, while pursuing a strategy of economic development that had the short-term consequence of increasing inequality in the distribution of income, the Pahlavi government would appear to have added fuel to the fire by following a semirepressive political development strategy that allowed opposition groups to organize but did not enable them to participate effectively.

If one takes income inequality and the repressiveness of the regime into account simultaneously, it might be argued that each variable could have an independent causal impact on the likelihood of mass political violence. An equally plausible specification of the joint relationship is that discontent resulting from income inequality will affect political violence only (or most strongly) in countries with semirepressive regime structures; whereas in countries with nonrepressive regime structures, inequality-induced discontent will tend to be channeled into peaceful participation; and in countries with repressive regime structures, it will be borne apathetically or else perhaps lead to various kinds of nonpolitical deviant behavior.

## A Causal Model

The diagram of Figure 1 includes the hypotheses elaborated above in the con-

Figure 1. Inequality and Insurgency: A Multivariate Causal Model


Size of Agricultural Labor Force
[Agrarian Inequality]
Land Inequality
Landlessness
Income Inequality
X
Semi-Repressive Regime
Governmental Acts of Coercion
Governmental Acts of Coercion (lagged)
Political Violence
Political Violence (lagged)
Level of Economic Development
Intensity of Separatism
+
+
+
+
+
+
+
+
+
+
−
−

text of a more comprehensive causal model. The set of inequality hypotheses is shown to the left of the dependent variable, mass political violence. Reading clockwise, hypotheses about the structure of the regime and the behavior of the incumbent political authorities are shown above the dependent variable; hypotheses referring specifically to time-lagged effects are to the right of it; and hypotheses about socioeconomic factors other than inequality are below it.

The argument that land inequality in agrarian societies is a major precondition of violence is represented by the arrow from agrarian inequality (defined as land inequality weighted by the size of the agricultural labor force) to political violence. It is important to weight land inequality by size of agricultural labor force because, as Huntington (1968, 381) observes, "in countries which have reached a high level of economic development, agriculture has a relatively minor role, and consequently even highly inequitable patterns of land ownership do not pose substantial problems of social equality and political stability." Thus, for example, it is implausible to expect that the relatively high Gini coefficients of land concentration in industrialized societies such as Australia and the United States, where the agricultural labor force is less than 10% of the population, could generate significant discontent (a point noted by Prosterman 1976, 350). The issue of land reform will be salient only if land inequality is high *and* the proportion of the labor force employed in agriculture is large. Therefore, the appropriate specification of the land-inequality hypothesis is an interaction between land inequality and the size of the agricultural labor force.

The model also includes the possibility that the proportion of the labor force that does not own or possess ownershiplike tenure of land (landlessness) is an alternative indicator of land maldistribution that may be correlated with land inequality but also could have an independent effect on political violence. Indeed, Prosterman (1976) argues that land inequality is a much less important cause of political violence and revolution than landlessness, and he regards landlessness as the single most salient cause of mass political violence in the twentieth century and a powerful predictive tool for anticipating which societies will have the greatest potential for revolution.

Our rival hypothesis, that agrarian inequality affects mass political violence only indirectly, depending on whether or not it contributes to an inegalitarian nationwide distribution of personal income, is represented by the arrows linking agrarian inequality to income inequality and income inequality to political violence. Similarly, we expect that landlessness will not have a significant direct effect on mass political violence and will be related to political violence only indirectly via the mediation of income inequality.

The expected relationship between political violence and the repressiveness of the regime can be expressed in two forms. If regime repressiveness is defined operationally as an ordinal typology consisting of the three properties, *nonrepressive*, *semirepressive*, and *very repressive*, then the hypothesis is simply that semirepressive regimes will be more likely to have high rates of political violence than either nonrepressive or very repressive regimes. This specification of the hypothesis, which facilitates the testing of an interaction between regime repressiveness and income inequality, is shown in Figure 1. If regime repressiveness is measured as a continuous variable, however, then an alternative specification is that violence will vary as an inverted U-curve of regime repressiveness (see Muller 1985).

In addition to the inequality and regime-repressiveness hypotheses, we include a hypothesis based on the distinction between the repressiveness of regime structures and acts of coercion undertaken by the political authorities that entail the use of negative sanctions to suppress the activity of dissident groups. On the one hand, extremely repressive regimes may deter insurgency by the threat of severe penalties, and the actual use of negative sanctions often may be relatively low. On the other hand, the political authorities in democratic regimes may on occasion engage in acts of coercion against dissidents who are considered to be a threat to internal security, and if such use of negative sanctions is limited in time and scope, the structure of the regime may remain nonrepressive. Therefore, it seems plausible to postulate that the coercive *behavior* of governments may vary more or less independently of the *structural* repressiveness of the regime. Experimental studies have shown that "aversive stimulation" in the form of insults and physical assault is the most potent elicitor of aggressive response in the laboratory (see Bandura 1973; Buss 1961). Tilly (1969) concludes from a historical survey of collective violence in Europe that coercive responses by political authorities to illegal but nonviolent action on the part of dissident groups frequently caused the level of protest to escalate from civil disobedience to violence. In addition, the seminal cross-national study of determinants of mass political violence by Hibbs (1973) found that the variable with by far the strongest direct—and positive—causal impact on variation in internal war (the sum of armed attacks, deaths from political violence, and assassinations) was governmental acts of coercion (called "negative sanctions"). Consequently, we hypothesize that regardless of the structural repressiveness of the regime, coercive actions taken by political authorities to suppress dissident groups will tend to instigate a violent response. This hypothesis is denoted by the arrow specifying a direct positive effect of governmental acts of coercion on political violence.

Hibbs's final model of causes of internal war cross-nationally for the 1948–67 period included a *negative* lagged effect of 1948–57 negative sanctions on 1958–67 internal war in addition to the positive instantaneous effect. Our model, which will be tested for the 1968–77 period, incorporates the possibility of a delayed deterrence effect, expressed by the negative sign attached to the arrow linking lagged governmental acts of coercion to political violence.

Hibbs also took lagged values of political violence into account. Inclusion of lagged values of the dependent variable in an explanatory model means that the results apply to change in that variable rather than to current level. We expect that our hypotheses about inequality and regime repressiveness will apply both to level of political violence and to change in political violence. Consequently, the model will be tested with and without a lagged political violence term—where lagged values refer to the first five-year interval of the period (1968–72) and current values refer to the second five-year interval (1973–77).

Political violence often occurs as a result of attempts by groups or regions to gain greater autonomy or to secede from the state. Relatively high rates of political violence during the 1970s resulting at least in part from the presence of intense separatist sentiment include such instances as the Catholics in Northern Ireland, the Muslims in the Philippines, blacks in South Africa and Rhodesia (now Zimbabwe), South Sudan in Sudan, and Baluchistan in Pakistan. We hypothesize that the intensity of separatist sentiment is a cause of political violence that can operate independently of the other variables in the model. Support for this hypothesis has been reported for earlier years by Hibbs (1973), where political separatism was found to be one of the four significant determinants of 1958–67 internal war in the final model.[6]

Level of economic development also is included in our model because Hardy (1979) and Weede (1981) have argued that political violence will vary cross-nationally as an inverse function of economic development and that income inequality has no effect on political violence independent of a country's development level. Consequently, one should control for level of economic development when testing inequality hypotheses in a multivariate context.

## A Cross-National Test of the Causal Model

There have been no studies reported to date that compare the causal importance of land maldistribution versus income inequality as determinants of mass political violence cross-nationally.[7] Until the 1970s, reasonably reliable information on the distribution of land and income was available for only a limited number of countries. Thus in Hibbs's (1973) comprehensive cross-national study of determinants of mass political violence during the 1948–67 period, inequality variables had to be excluded because of insufficient data. We now have been able to compile a relatively comprehensive data set on inequality circa 1970 (see Appendix). Information on land inequality is available for approximately three-quarters of the population of independent political units in 1970, while information on landlessness and income inequality is available for approximately one-half of the population. Regionally, these data are quite comprehensive for Europe and the Americas. In regard to landlessness and, especially, income distribution, coverage is poor for states in the Middle East and North Africa, and it is somewhat limited for the states of sub-Saharan Africa. Since it is unlikely that much new data on inequality circa 1970 will emerge in the future, results using the current data set can probably be regarded as being about as definitive as possible for this time period.

### Measurement of the Dependent Variable

Political violence is measured by the natural logarithm of the death rate from domestic conflict per one million population.[8] Annual death counts are from Table 2.7 of Taylor and Jodice (1983, vol. 2). Current political violence is the logged sum of annual deaths from domestic political conflict during 1973–77 divided by midinterval population; lagged political violence is the logged sum of annual deaths from domestic political conflict during 1968–72 divided by midinterval population. Countries where domestic political conflict overlaps with major interstate wars are excluded: Kampuchea, Laos, and South Vietnam for the 1968–77 period; and Pakistan for the 1968–72 period (where an extremely high death rate reflects the conflict between India and Pakistan in 1971 over the secession of Bangladesh). Ireland also is excluded for the 1973–77 period because the relatively high death rate there reflects a spillover from the Northern Ireland conflict.

In the vast majority of countries, the death rate from political violence per one million population is less than 50. A few countries register very extreme scores, however; for example, Zimbabwe's 1973–77 death rate from political violence was 544 per million and Argentina's death rate was 177 per million. Even after logging, countries with political violence death rates of 50 or more almost always show up as outliers in regression equations (i.e., they usually have extremely high standardized residuals). Consequently, in order to reduce the problem of extreme scores on the dependent variable, it is desirable to set a ceiling on the death rate. The upper limit that we have selected is 50 deaths per million. The adjusted death rate variables thus range from a minimum value of 0 to a maximum value of 50 or more; and the range of the logged death rate variables is from 0 to 3.93.

### Measurement of the Independent Variables

The data on land inequality circa 1970 encompass 85 states in which agriculture was not collectivized. Land inequality is measured by the Gini coefficient of land concentration. A weighted index of land inequality is the geometric mean of the Gini coefficient (expressed as a percentage) and the percentage of the labor force employed in agriculture in 1970 (see Taylor and Jodice 1983, vol. 1). Apart from measurement of the extent to which land is concentrated in the hands of the few, we also take into account a second aspect of land maldistribution, landlessness, as measured by agricultural households without land as a proportion of the total labor force. These data are derived from estimates by Prosterman and Riedinger (1982) of the proportion in 64 countries of agricultural households without land.

Income inequality is measured by the size of the share of personal income accruing to the richest quintile of recipients, based on information about the nationwide distribution of income in 63 countries compiled principally from publications of the World Bank. Although some previous studies have used Gini coefficients of income concentration, this measure tends to be unduly sensitive to inequality in the middle of the distribution, whereas inequality in reference to the top of the distribution probably is more relevant to political violence. In any event, income shares also have a more direct meaning than Gini coefficients and are currently more frequently used in research on income inequality.

Regime repressiveness is measured by a country's 1973–77 average annual combined rating on 7-point rank-order scales of political rights and civil liberties that have been reported by Raymond D. Gastil since 1973 (the data are from Taylor and Jodice 1983). A semirepressive regime structure is defined operationally as a mean political rights and civil liberties rank in the range of 2.6–5.5. These cutpoints are identical to those used by Gastil for classifying political systems as "free" (1.0–2.5), "partly free" (2.6–5.5), and "not free" (5.6–7.0).

The indicator of governmental acts of

coercion is the negative sanctions variable (imposition of sanctions) from Taylor and Jodice 1983 (vol. 2, tbl. 3.1). Current negative sanctions is the frequency of negative sanctions summed over the years 1973–77 and divided by midinterval total populatioan in millions; lagged negative sanctions is the 1968–72 frequency per one million midinterval population. The negative sanctions variables are expressed as natural logarithms (after adding an increment of one).

The indicator of intensity of separatism is an ordinal scale developed by Ted and Erika Gurr. The data for circa 1975 are from Taylor and Jodice 1983, 55–57 and tbl. 2.5. We express intensity of separatism as a dummy variable, scored 1 (i.e., high intensity) if groups or regions actively advocating greater autonomy were forcibly incorporated into the state (codes 3 and 4) and 0 (i.e., low intensity) otherwise (codes 0, 1, and 2).[9]

Level of economic development is measured by energy consumption per capita in 1970 (from Taylor and Jodice 1983, vol. 1). Values of this variable are expressed as natural logarithms.

## Land Maldistribution, Income Inequality, and Political Violence

According to what is generally considered to be the most appropriate specification of the land inequality hypothesis (e.g., Huntington 1968; Nagel 1976; Prosterman 1976), the strongest effect on political violence should be observed when inequality in the distribution of land is weighted by the proportion of the labor force employed in the agricultural sector of the economy. This specification implies a multiplicative interaction between land inequality and the size of the agricultural labor force, which we call *agrarian inequality*, defined operationally as the geometric mean of Gini land concentration and the percentage of the labor force employed in agriculture (i.e., the square root of the product of these variables). When the 1973–77 death rate from political violence (denoted *lnDPM75*) is regressed on agrarian inequality for 83 cases,[10] the result is

$$lnDPM75 = 0.29 + \underset{(2.27)}{0.017(\mathrm{AGINEQUAL})} \qquad (1)$$

$$R_a^2 = .05$$

Although the size of the t-ratio (in parentheses) indicates that the parameter estimate for agrarian inequality is statistically significant at the .05 level, the magnitude of $R_a^2$ (the adjusted accuracy of prediction —an estimate of $R^2$ for the population) is trivial. Indeed, the correlation ($r$) of .24 between the political-violence death rate and agrarian inequality is weaker than that for unweighted Gini land concentration, which is .27. Thus, weighting Gini land concentration by size of agricultural labor force, a procedure that theoretically should enhance the correlation, in fact reduces accuracy of prediction.[11]

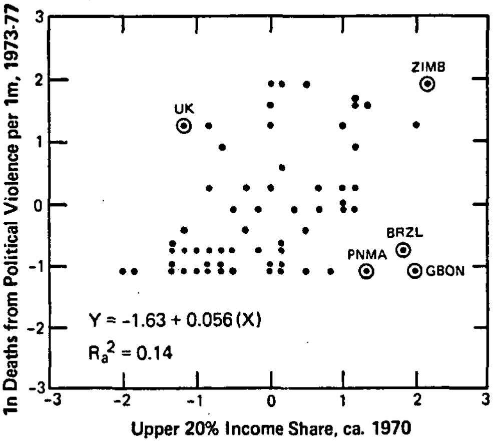
If the hypothesis of an effect of landlessness on the 1973–77 death rate from political violence is taken into account in addition to Gini land concentration, the regression equation estimated across 60 countries is

$$lnDPM75 = 0.46 + \underset{(0.71)}{0.62(\mathrm{GINILAND})} + \underset{(1.51)}{0.023(\mathrm{LANDLESS})} \qquad (2)$$

$$R_a^2 = .03$$

The t-ratios show that for countries with information on both of the land maldistribution variables, the parameter estimate for landlessness is of borderline significance (between the .05 and .10 levels for a one-tailed test), while that for Gini land concentration is not significant. This finding supports the argument of Prosterman (1976) that the proportion of the labor force without land is a more relevant determinant of political violence than the extent to which landholdings are unequally distributed. When Equation 1 is

**Figure 2. Plot of Standardized Death Rates from Political Violence by Standardized Income Inequality Scores**



reestimated with the nonsignificant Gini land-concentration variable deleted, the t-ratio for the landlessness parameter estimate rises to 1.87, which is significant at less than .05 for a one-tailed test. $R_a^2$ remains trivial, however, increasing only to .04. The strength of the correlational support for Prosterman's argument thus is quite weak in cross-national perspective.[12]

If inequality in the distribution of income—defined operationally by the share of personal income accruing to the upper quintile of recipients—is taken into account in addition to landlessness, the regression equation for 47 countries with information on both variables is

$$\begin{aligned} lnDPM75 = -1.40 &+ \underset{(0.62)}{0.011(\text{LANDLESS})} \\ &+ \underset{(2.03)}{0.048(\text{U20INCOME})} \qquad (3) \\ R_a^2 = .12 & \end{aligned}$$

In this equation the effect of landlessness is reduced to nonsignificance while the effect of income inequality is estimated to be significant at the .05 level. Thus, on controlling for income inequality, landlessness becomes an irrelevant variable.

The correlation between the 1973–77 death rate from political violence and income inequality is .39 for 62 cases—a moderate degree of fit to a linear trend.[13] As can be seen from the scatterplot shown in Figure 2, most countries with relatively low income inequality (an upper 20% share that is one-half of a standard deviation or more below the mean) have relatively low death rates from political violence (in the range of 0 to −1 standard deviation), whereas most countries with relatively high income inequality (an upper 20% share that is one standard deviation or more above the mean) have relatively high death rates (equal to or above the mean). The relationship is subject to heteroskedasticity, however, because Brazil, Gabon, and Panama have very low death rates from political violence despite income-inequality scores greater than one standard deviation above the mean. These countries comprise three of the five most influential cases in the bivariate regression (circled in Figure 2), and, because they are deviant cases, their presence reduces accuracy of prediction.[14]

The United Kingdom also is an influential deviant case because it has a relatively high death rate from political violence despite an income inequality score that is more than one standard deviation below the mean. In the following multivariate regressions, we want to analyze the extent to which these deviant cases can be explained by taking into account the set of variables postulated to have additional effects on political violence independent of income inequality.

## The Multivariate Model

We turn now to the question of whether the relationship between income inequality and mass political violence holds cross-nationally in the context of a multivariate equation that takes into account the explanatory variables shown to the right of income inequality in the model of Figure 1. Table 1 reports the parameter estimates[15] obtained from the regression of the 1973–77 death rate from political violence on income inequality and the other explanatory variables, including the lagged political-violence death rate (Equations 1.1 and 1.2) and excluding it (Equations 1.3 and 1.4).[16]

The hypothesis of a direct effect of income inequality on change in political violence is confirmed in Equation 1.1. Otherwise, the effects are as predicted in the model of Figure 1 except for that of energy consumption (the indicator of level of economic development), which is estimated to have a positive instead of a negative effect on change in political violence. The expected negative effect of lagged negative sanctions (the indicator of governmental acts of coercion) is not significant at the .05 level, however.

### Table 1. Regressions of Death Rate from Political Violence on Income Inequality and Other Explanatory Variables

| | ln Deaths from Political Violence per 1m, 1973–77 | | | |
|---|---|---|---|---|
| | Change | | Level | |
| Explanatory Variables | 1.1 | 1.2 | 1.3 | 1.4 |
| Intercept | -4.02 | -3.81 | -2.55 | -1.56 |
| Upper 20% income share, ca. 1970 | 0.046*<br>(2.95)<br>.32 | 0.045*<br>(2.83)<br>.31 | 0.038*<br>(2.29)<br>.27 | 0.034*<br>(2.31)<br>.24 |
| ln energy consumption per capita, 1970 | 0.25<br>(2.32)<br>.29 | 0.22<br>(2.07)<br>.25 | 0.11<br>(1.03)<br>.13 | |
| Intensity of separatism, 1975 | 0.87*<br>(2.51)<br>.24 | 0.86*<br>(2.44)<br>.23 | 1.23*<br>(3.42)<br>.35 | 1.24*<br>(3.60)<br>.35 |
| Semirepressive regime, 1973–77 | 0.86*<br>(2.93)<br>.33 | 0.83*<br>(2.81)<br>.32 | 1.23*<br>(3.08)<br>.37 | 0.82*<br>(2.96)<br>.31 |
| ln negative sanctions per 1m, 1973–77 | 0.54*<br>(2.51)<br>.28 | 0.39*<br>(2.09)<br>.20 | 0.33<br>(1.47)<br>.17 | 0.46*<br>(2.44)<br>.23 |
| ln negative sanctions per 1m, 1968–72 | -0.38<br>(-1.37)<br>- .17 | | 0.13<br>(0.50)<br>.06 | |
| ln deaths from political violence, 1968–72 | 0.48*<br>(3.54)<br>.42 | 0.38*<br>(3.27)<br>.34 | | |
| $R_a^2$ | .55 | .54 | .46 | .46 |
| Number of cases | 60 | 60 | 61 | 62 |

Note: t-ratio in parentheses; standardized regression coefficient underscored.
*$p < .05$, one-tailed.

When the nonsignificant lagged negative-sanctions variable is removed, the results of Equation 1.2 are quite similar to those of 1.1. (Energy consumption is retained in 1.2 because it is significant in 1.1 at the .05 level for a two-tailed test.)

The regressions for explaining variation in level of political violence are reported in Equations 1.3 and 1.4. The hypothesis of a direct effect of income inequality on level of political violence is confirmed in Equation 1.3. The unexpected positive effect of energy consumption is weak (and not significant at the .05 level for a two-tailed test) in 1.3, while the effect of lagged negative sanctions changes from negative to positive but is very weak and nonsignificant. Otherwise, the effects are as predicted both in Equation 1.3 and in Equation 1.4, where nonsignificant variables are removed.

The unexpected and implausible positive relationship between economic development and political violence is thus observed only in regard to change in the political-violence death rate. The positive income inequality effect, as well as the positive effects of high intensity of separatism, the presence of a semirepressive regime structure, and the rate of negative sanctions,[17] hold in regard to explanation of variation in change in and in level of the political-violence death rate.

The equations of Table 1 specify an additive effect of income inequality on political violence. Is this the correct specification? Is the magnitude of the inequality effect more or less the same across all countries, regardless of their regime structure?

A strong version of the alternative inequality-repressiveness-interaction hypothesis entails two predictions: (1) that the product of income inequality and the semirepressive-regime dummy variable will have a significant direct effect on political violence and (2) that income inequality alone will not have a significant direct effect on political violence. A weaker version of the inequality-repressiveness-interaction hypothesis entails only the first prediction. Regressions that test the hypothesis of an interaction between income inequality and a semirepressive regime are reported in Table 2. The possibility of an independent effect of a semirepressive regime cannot be evaluated because of multicollinearity: this variable correlates with the product of itself and income inequality at $r = .99$ for the 60 countries and at $r = .98$ for the 62 countries used to estimate the equations for explaining change in and level of political violence, respectively.

Equation 2.1 includes the inequality × semirepressiveness product term in addition to the significant variables from Equation 1.2. Equation 2.2 excludes the possibility of an additive effect of income inequality. The additive effect is significant in Equation 2.1. Exclusion of it in 2.2 reduces $R_a^2$. Support thus is observed for the specification of an additive effect of income inequality on change in political violence. The product term is also significant in Equation 2.1. This finding indicates that the presence of a semirepressive regime may *enhance* the strength of the income-inequality effect. $R_a^2$ for 2.1 is identical, however, to that for 1.2. Therefore, the product term is a superfluous variable. The results of Equations 2.3 and 2.4 (re level of political violence) are similar to those of 2.1 and 2.2 but slightly less clearcut. On the one hand, the strength of the additive effect of income inequality in 2.3 is weaker than in 2.1; on the other hand, inclusion of the product term produces only a trivial increase in $R_a^2$ (from .46 in Equation 1.3 to .47 in Equation 2.3). In sum, (1) there is no evidence to indicate that the additive specification is incorrect, but (2) there is also a possibility that the effect of income inequality on political violence may be greater in semirepressive regimes than in nonrepressive or in extremely repressive regimes.

Another important issue is the question of the robustness of the income inequality effect. Two tests of robustness will be performed. First, we will assess the extent to which the income-inequality parameter estimates from Equations 1.2 and 1.4 are sensitive to the presence of influential cases. Then we will determine whether the inequality effects hold across a more restrictively defined sample.

According to Cook's D,[18] the most influential case in Equation 1.2 is Sudan, which registers a value of .18 (the next highest values are .14 and .13, registered by Indonesia and Panama, respectively). Equation 3.1 of Table 3 shows that when Sudan is deleted, the unstandardized and standardized regression coefficients for income inequality remain virtually unchanged (.46 and .33) in comparison with the full sample estimates (.045 and .31) from Equation 1.2. In Equation 1.4 two cases are most influential: Spain, with a Cook's D of .24, and Indonesia, with a Cook's D of .23 (the next highest values are .15 and .13, registered by Portugal and Sudan, respectively). Equation 3.3 shows that when Spain and Indonesia are

**Table 2. Regressions of Political Violence and Income Inequality with Tests for Interaction with the Presence of a Semirepressive Regime**

| | ln Deaths from Political Violence per 1m, 1973–77 | | | |
|---|---|---|---|---|
| | Change | | Level | |
| Explanatory Variables | 2.1 | 2.2 | 2.3 | 2.4 |
| Intercept | -3.41 | -1.00 | -1.21 | 0.028 |
| Upper 20% income share, ca. 1970 | 0.038*<br>(2.31)<br>.26 | | 0.027*<br>(1.73)<br>.19 | |
| Upper 20% income share × semirepressive regime | 0.016*<br>(2.93)<br>.34 | 0.020*<br>(3.94)<br>.43 | 0.016*<br>(3.07)<br>.34 | 0.021*<br>(4.57)<br>.44 |
| ln energy consumption per capita, 1970 | 0.20<br>(1.97)<br>.23 | 0.11<br>(1.13)<br>.13 | | |
| Intensity of separatism, 1975 | 0.83*<br>(2.39)<br>.23 | 0.80*<br>(2.20)<br>.22 | 1.24*<br>(3.62)<br>.35 | 1.20*<br>(3.45)<br>.34 |
| ln negative sanctions per 1m, 1973–77 | 0.42*<br>(2.30)<br>.21 | 0.43*<br>(2.26)<br>.22 | 0.48*<br>(2.54)<br>.24 | 0.47*<br>(2.47)<br>.24 |
| ln deaths from political violence, 1968–72 | 0.39*<br>(3.34)<br>.34 | 0.37*<br>(3.06)<br>.33 | | |
| $R_a^2$ | .54 | .51 | .47 | .45 |
| Number of cases | 60 | 60 | 62 | 62 |

Note: t-ratio in parentheses; standardized regression coefficient underscored.
*p < .05, one-tailed.

deleted, the unstandardized and standardized regression coefficients for income inequality are smaller (.027 and .19) than the full-sample estimates (.034 and .24) from Equation 1.4, but the income-inequality effect in 3.3 nevertheless remains significant at the .05 level. Thus the effect of income inequality on political violence is always statistically significant, regardless of influential cases, but its estimated causal weight in regard to explanation of variation in the level of political violence is enhanced somewhat by the presence of influential cases.[19]

The second test of robustness is severe because it entails eliminating almost one-third of the sample. Our primary source of information on income distribution is the *World Development Report*, published annually by the World Bank since 1978. These data have been supplemented with information from other sources—

**Table 3. Regressions of Political Violence and Income Inequality with Tests of the Robustness of the Inequality Effect**

| | ln Deaths from Political Violence per 1m, 1973–77 | | | |
|---|---|---|---|---|
| | Change | | Level | |
| Explanatory Variables | 3.1$^a$ | 3.2$^b$ | 3.3$^a$ | 3.4$^b$ |
| Intercept | -4.13 | -3.69 | -1.26 | -1.42 |
| Upper 20% income share, ca. 1970 | 0.046* | 0.040* | 0.027* | 0.035* |
| | (2.97) | (1.76) | (1.96) | (1.67) |
| | .33 | .27 | .19 | .24 |
| ln energy consumption per capita, 1970 | 0.25 | 0.22 | | |
| | (2.43) | (1.73) | | |
| | .30 | .29 | | |
| Intensity of separatism, 1975 | 0.68* | 1.05* | 1.69* | 1.38* |
| | (1.93) | (2.40) | (4.98) | (3.10) |
| | .18 | .30 | .44 | .39 |
| Semirepressive regime, 1973–77 | 1.01* | 0.84* | 0.97* | 0.62* |
| | (3.34) | (2.34) | (3.76) | (1.79) |
| | .39 | .36 | .37 | .25 |
| ln negative sanctions per 1m, 1973–77 | 0.39* | 0.40* | 0.50* | 0.40* |
| | (2.16) | (1.91) | (2.62) | (1.81) |
| | .21 | .23 | .23 | .23 |
| ln deaths from political violence, 1968–72 | 0.32* | 0.44* | | |
| | (2.70) | (3.07) | | |
| | .28 | .40 | | |
| $R_a^2$ | .54 | .48 | .56 | .37 |
| Number of cases | 59 | 43 | 60 | 44 |

Note: t-ratio in parentheses; standardized regression coefficient underscored.

*$p < .05$, one-tailed.

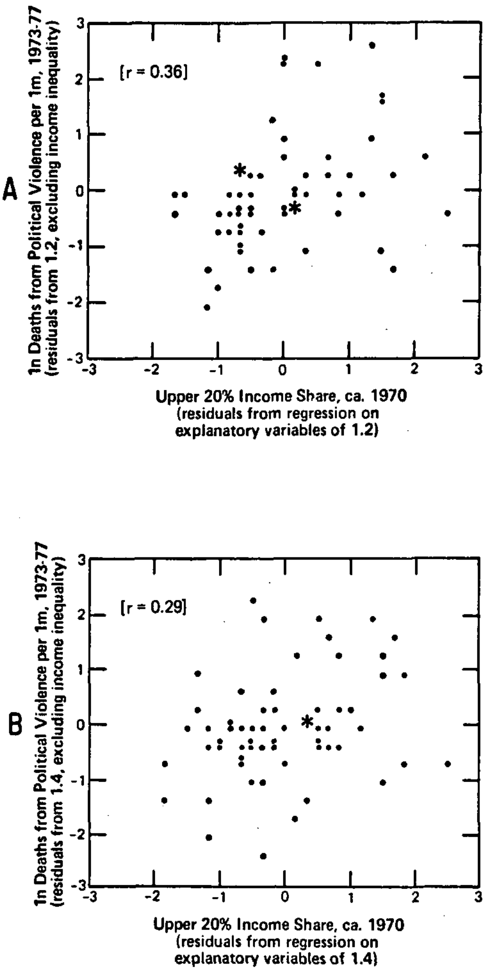
$^a$Most influential case(s) deleted.

$^b$Excluding countries with data on income distribution exclusively from sources other than the World Bank's *World Development Report*.

including previous World Bank compilations—that seem to be reliable. From Table A-1 one may observe that 18 countries have data on income distribution exclusively from sources other than the *World Development Report* (denoted *W*). We have included them in the interest of maximizing sample size. Let us see if excluding them makes a difference.[20]

Equation 3.2 includes the same variables as 1.2 but is estimated across 43 instead of 60 cases.[21] Although the t-ratio for the income-inequality parameter estimate is substantially reduced, it still indicates the presence of a significant effect at the .05 level. More important, the unstandardized and standardized regression coefficients for income inequality in Equation 3.2 are only slightly smaller than the full-sample estimates. Equation 3.4 includes the same variables as 1.4 but is estimated across 44 instead of 62 cases. Again, although the t-ratio for the income-inequality effect is substantially reduced, the null hypothesis still can be rejected with 95% confidence. And in this equation the unstandardized and standardized regression coefficients for income inequality are virtually identical with those of the full sample.

**Figure 3. Standardized Partial Plots of Political Violence Death Rate Residuals from Equations 1.2 and 1.4 (Excluding Income Inequality) by Income Inequality Residuals (Income Inequality Regressed on the Other Explanatory Variables)**

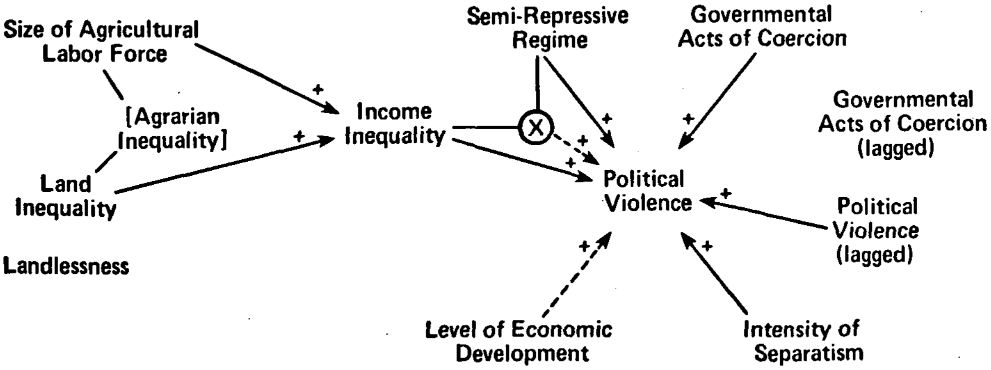


A final question that should be considered is whether heteroskedasticity remains a problem. We observed from the scatterplot of Figure 2 that heteroskedasticity was present in the bivariate relationship between income inequality and the death rate from political violence. Plot A of Figure 3[22] shows a standardized partial plot of the political violence residuals by the income-inequality residuals, when both variables are regressed on the other explanatory variables from Equation 1.2. The correlation between these residuals ($r = .36$) is equal to the partial correlation between income inequality and the 1973–77 death rate from political violence, controlling for the other explanatory variables. If heteroskedasticity has been removed by inclusion of additional variables, the standardized partial residual plot should be approximately linear; whereas, a fan-shaped joint distribution would indicate that heteroskedasticity had not been removed. The observed joint distribution of scores clearly is linear instead of fan-shaped, so we conclude that the heteroskedasticity in the relationship between income inequality and political-violence death rate has been substantially reduced by taking the other explanatory variables into account.[23]

Plot B of Figure 3 shows a standardized partial plot of the political-violence residuals by the income-inequality residuals, when both variables are regressed on the other explanatory variables from Equation 1.4. The distribution of scores in plot B also conforms to a linear trend rather than being fan-shaped, so it appears that, in the multivariate context, heteroskedasticity has again been more or less eliminated.

The results of testing the inequality hypotheses in the context of a multivariate model of determinants of political violence are summarized in Figure 4. All of the evidence that we have considered points to the presence of a robust, positive monotonic (positively accelerated) relationship between income inequality and political violence that is independent of the other variables in the model. The effect of income inequality on political violence may be enhanced by the presence of a semirepressive regime, but the evidence is not conclusive in that regard, so we represent the possibility of an interaction between income inequality and semirepressiveness by dashed arrows. The other solid arrows linking explanatory variables to political violence also denote relationships that hold for change as well as level of violence and seem to be robust. We have tested the regime-repressiveness hypothesis with a dummy variable in this study (in order to take into account the possibility of an interaction with income inequality). It should be noted, however, that the same kind of effect appears if regime repressiveness is expressed as a continuous quantitative variable—that is, if the semirepressive-regime dummy variable is replaced by regime repressiveness and its square, a statistically significant *nonmonotonic-inverted-U-curve* relationship between regime repressiveness and political violence is consistently observed in multivariate equations that include income inequality and the other explanatory variables. We have not tested for the possibility of an instantaneous reciprocal relationship between political violence and governmental acts of coercion (see Hibbs 1973) because that is a complex topic requiring a separate paper. From preliminary work, however, we are confident that it is valid to infer the pres-

**Figure 4. Observed Causal Paths in the Multivariate Causal Model**


Size of Agricultural Labor Force
[Agrarian Inequality]
Land Inequality
Landlessness
Income Inequality
Semi-Repressive Regime
Governmental Acts of Coercion
Governmental Acts of Coercion (lagged)
Political Violence
Political Violence (lagged)
Level of Economic Development
Intensity of Separatism
X
+
+
+
+
+
+
+
+
+

ence of a positive effect of current governmental acts of coercion on current political violence.

The causal paths linking the maldistribution of land variables to income inequality are inferred from the following regression equations:

$$\text{U20INCOME} = 33.60 + \underset{(5.09)}{0.33}(\text{AGINEQUAL}) + \underset{(0.28)}{0.028}(\text{LANDLESS}) \quad (4)$$

$$R_a^2 = .55 \qquad N = 46$$

$$\text{U20INCOME} = 33.02 + \underset{(8.10)}{0.36}(\text{AGINEQUAL}) \quad (5)$$

$$R_a^2 = .55 \qquad N = 54$$

$$\text{U20INCOME} = 26.56 + \underset{(5.85)}{23.96}(\text{GINILAND}) + \underset{(7.40)}{0.21}(\%\text{AGLABOR}) \quad (6)$$

$$R_a^2 = .60 \qquad N = 54$$

In Equation 4, which compares the effects on income inequality of agrarian inequality and landlessness, landlessness is an irrelevant variable (the t-ratio is virtually zero). Equations 5 and 6 compare the multiplicative index of agrarian inequality with its components (the size of the agricultural labor force is denoted *%AGLABOR*) as determinants of income inequality. Since the $R_a^2$ for the multiplicative model (5) is lower than that for the additive model (6), we infer that land inequality and size of agricultural labor force have independent additive effects on income inequality instead of a multiplicative-interaction effect.

The only completely irrelevant variable in the model is landlessness, a finding that runs counter to the strong claim of causal importance for this variable made by Prosterman (1976). Moreover, at least as a general determinant of mass political violence, the condition of high agrarian inequality also fails to warrant the strong causal claims made for it by many scholars. The components of agrarian inequality, land inequality and size of the agricultural labor force, affect income inequality and, therefore, are indirectly relevant to political violence, but neither the weighted index of agrarian inequality nor land inequality per se has any direct effect on political violence.

## Discussion

The finding that agrarian inequality is relevant only to the extent that it is associated with inequality in the nationwide distribution of income has important policy implications. Land reform in third world countries all too often is considered to be a panacea for problems of inequality. However, as Huntington (1968, 385) points out, redistribution of land is the most difficult of reforms for modernizing governments because it almost always entails some degree of outright confiscation. And our study indicates that land redistribution is also not necessarily the most meaningful of reforms. If land redistribution is carried through to the point of actually effecting an egalitarian redistribution of income, as seems to have been the case in countries as diverse as Taiwan and Egypt, and/or if other economic development policies do not exacerbate income inequality, then land reform can make a contribution to the promotion of political stability. However, there are cases such as Bolivia and Mexico in which land reform has not been associated with egalitarian income redistribution. Land reform without income redistribution is probably at best merely a temporary palliative; and at worst, as the case of Iran demonstrates, it can be quite counterproductive by alienating powerful conservative groups such as the nobility and the clergy. Indeed, by simultaneously encouraging both land reform and a policy of rapid economic growth that ignored inegalitarian distributional consequences, U.S. advisors to the shah would appear unwittingly to have exacerbated the economic preconditions of revolution in Iran.

If the effect of income inequality on change in political violence and its level, observed for 60 and 62 cases, is reliable and more or less generalizable across time in the contemporary world (at least for nontraditional societies where modern values like equality can be assumed to have become salient), it follows that redistribution of income must be ranked as one of the more meaningful reforms that a modernizing government can undertake in the interest of achieving political stability. Unfortunately, redistribution of income may conflict not only with the class interests of many third world governments but also with their predilection for rapid industrialization. The shah's great dream of surpassing Sweden by the year 2000 was dashed in part by his single-minded concern with economic growth and the raising of per capita income. As Green (1982, 70–71) points out, "the premise of the Pahlavi development ethos rested on the assumption that economic development was more important than political rights or justice." Iran in the years immediately preceding the revolution indeed registered an extraordinary growth of per capita gross national product, which averaged an increase of 13.3% annually during 1970–78, the highest rate of growth of GNP per capita in the world (see Taylor and Jodice 1983, vol. 1, tbl. 3.6); but at the same time that per capita income was increasing phenomenally, the distribution of that income was apparently becoming more concentrated at the top, presumably heightening perceptions of economic injustice. It is important to emphasize, however, that there is no necessary trade-off between rapid economic growth and income inequality. Taiwan's average annual growth of GNP per capita during 1960–78 was 6.6% (see World Bank 1980, tbl. 1), a rate that, although surpassed by Iran (the world leader excluding Romania), was nevertheless almost twice as high as the average rate (3.7%) for all middle-income countries. At the same time (1964–78), the income share of the richest 20% of households in Taiwan declined from 41.1% to 37.2% (see Tsiang 1984, tbl. 9). Thus, by following a different set of economic policies than the shah, the government of Taiwan achieved growth with equity. And the death rate from political violence in Taiwan during 1973–77 was .06, as compared with Iran's rate of .91.

Political rights also are a relevant source of stability and instability, not only as was the case in Iran but, as it appears from our cross-national results, globally. The problem faced by modernizing governments in this regard is that political rights must either be granted fully, in which case the government is allowing for the real possibility of being voted out of power, or not be granted at all, in which case the government must enforce a degree of totalitarian control over the populace that is costly to maintain and is probably inherently at odds in the long run with a capitalist economic system.

Another problem faced by all governments is that the use of coercion against dissidents—closing down their newspapers, preventing them from assembling in public, and imprisoning them or killing them if they disobey—seems in the short run to provoke violence rather than deter it. If the political system is open and liberal, however, the rate of coercion-provoked violence will usually not reach regime-threatening proportions because the presence of meaningful nonviolent possibilities of influencing the political process will inhibit the ability of revolutionary-minded dissidents to mobilize large followings. Also, if the political system is so repressive that dissidents have little or no opportunity to organize, then coercion-provoked violence probably will not become regime threatening. The worst strategy for preventing regime-threatening rates of mass political violence would seem to be that of attempting to suppress opposition by acts of coercion while at the same time maintaining a semirepressive regime structure that permits some organization and expression of discontent but does not give dissident groups genuine opportunities to participate effectively in political decision making. Were this strategy to be pursued by the political leadership in countries with high levels of income inequality and/or a high potential for separatism, the results of our cross-national analysis indicate that the likelihood of a high level of political violence and protracted civil war would become very strong.

## Appendix

Data on land inequality, landlessness, and income inequality in independent states are listed by region in Table A-1.

**Table A-1. Distribution of Land and Income Circa 1970**

| | Land Concentration | | Agricultural Households without Land as % of Total Labor Force | Income Share of Upper 20% | |
|---|---|---|---|---|---|
| | Year | Gini | | Source and Year | % |
| Europe & North America | | | | | |
| Austria | 1970 | .70 | 3.6 | | |
| Belgium | 1970 | .59 | 0.4 | *W* 1974–75 | 39.8 |
| Canada | 1971 | .51 | 1.6 | *W* 1969, 1977 | 41.5 |
| Denmark | 1970 | .43 | 2.0 | *W* 1976 | 37.5 |
| Finland | 1970 | .25 | 1.3 | *W* 1977 | 36.8 |
| France | 1971 | .53 | 2.9 | *W* 1970, 1975 | 46.4 |
| Germany, East | collectivized | | | *A* 1970 | 30.7 |
| Germany, West | 1971 | .51 | 0.6 | *S* 1970; *W* 1973, 1974 | 45.5 |
| Greece | 1971 | .48 | 4.1 | | |
| Hungary | collectivized | | | *A* 1969 | 33.4 |
| Ireland | 1970 | .49 | 4.5 | *W* 1973 | 39.4 |
| Italy | 1970 | .75 | 4.0 | *W* 1969, 1977 | 45.2 |
| Luxembourg | 1970 | .46 | | | |
| Malta | 1969 | .53 | | | |
| Netherlands | 1970 | .47 | 0.4 | *W* 1967, 1975, 1977 | 39.0 |
| Norway | 1969 | .30 | 0.7 | *W* 1970 | 37.3 |
| Poland | 1970 | .46 | 5.1 | | |
| Portugal | 1968 | .81 | 7.3 | *W* 1973–74 | 49.1 |
| Spain | 1960 | .80 | 9.6 | *W* 1974 | 42.2 |
| Sweden | 1971 | .23 | 0.9 | *W* 1972 | 37.0 |
| Switzerland | 1969 | .51 | 2.0 | *B* 1968 | 45.9 |
| United Kingdom | 1970 | .69 | 0.8 | *S* 1972; *W* 1973 | 39.1 |
| United States | 1969 | .72 | 1.3 | *W* 1972 | 42.8 |
| Yugoslavia | 1969 | .56 | 3.5 | *A* 1968; *W* 1973 | 40.8 |
| Central & South America | | | | | |
| Argentina | 1960 | .87 | 8.2 | *W* 1970 | 50.3 |
| Barbados | | | | *J* 1969–70 | 44.0 |
| Bolivia | | | 12.8 | *P* 1968 | 61.0 |
| Brazil | 1970 | .84 | 29.2 | *W* 1972 | 66.6 |
| Chile | | | | *W* 1968 | 51.4 |
| Colombia | 1971 | .86 | 13.6 | *A* 1970 | 59.4 |
| Costa Rica | 1973 | .82 | 24.5 | *T* 1971, 1974, 1977 | 52.1 |
| Dominican Republic | 1971 | .82 | 31.2 | | |

TABLE A-1 (continued)

| | Land Concentration | | Agricultural Households without Land as % of Total Labor Force | Income Share of Upper 20% | |
|---|---|---|---|---|---|
| | Year | Gini | | Source and Year | % |
| Ecuador | 1974 | .82 | | | |
| El Salvador | 1971 | .81 | 39.8 | *J* 1965–67, 1969; *W* 1976–77 | 52.1 |
| Guatemala | 1964 | .82 | 17.7 | | |
| Haiti | 1971 | .47 | | | |
| Honduras | 1974 | .78 | | *W* 1967 | 67.8 |
| Jamaica | 1969 | .80 | 8.9 | *A* 1958; *US* 1982 | 58.3 |
| Mexico | 1970 | .93 | 16.7 | *A* 1969; *W* 1977 | 59.2 |
| Nicaragua | 1963 | .80 | | *L* 1977 | 60.0 |
| Panama | 1971 | .78 | 36.2 | *W* 1970 | 61.8 |
| Peru | 1972 | .91 | 10.8 | *W* 1972 | 61.0 |
| Trinidad & Tobago | 1963 | .69 | | *W* 1975–76 | 50.0 |
| Uruguay | 1970 | .82 | 8.4 | *A* 1967 | 47.4 |
| Venezuela | 1971 | .91 | 5.9 | *W* 1970 | 54.0 |
| Middle East & North Africa | | | | | |
| Algeria | 1973 | .65 | 0 | | |
| Egypt | 1964 | .67 | 16.3 | *W* 1974 | 48.0 |
| Iran | 1960 | .63 | | | |
| Iraq | 1971 | .61 | 20.0 | | |
| Israel | 1971 | .75 | | | |
| Jordan | | | 21.0 | | |
| Kuwait | 1970 | .75 | | | |
| Lebanon | 1970 | .77 | | | |
| Libya | 1960 | .70 | | | |
| Saudi Arabia | 1974 | .78 | 17.2 | | |
| Syria | 1971 | .67 | | | |
| Tunisia | | | | *A* 1970 | 55.0 |
| Turkey | 1963 | .59 | 24.4 | *W* 1973 | 56.5 |
| Sub-Saharan Africa | | | | | |
| Botswana | 1969 | .47 | | | |
| Cameroon | 1973 | .42 | | | |
| Central African Republic | 1974 | .34 | 6.4 | | |
| Chad | 1973 | .34 | 3.6 | | |
| Congo | 1973 | .27 | 2.9 | | |
| Gabon | 1975 | .41 | | *A* 1968 | 67.5 |
| Ghana | 1970 | .55 | 14.6 | *R* 1968 | 47.8 |
| Ivory Coast | 1975 | .42 | | *A* 1970 | 57.2 |
| Kenya | 1974 | .67 | 12.3 | *W* 1976 | 60.4 |
| Lesotho | 1970 | .36 | 18.8 | | |
| Liberia | 1971 | .73 | | | |
| Malawi | 1969 | .34 | 8.9 | *W* 1967-68 | 50.6 |
| Mali | 1960 | .48 | | | |
| Nigeria | | | 6.2 | | |
| Sierra Leone | 1971 | .43 | 17.2 | *W* 1967-69 | 52.5 |
| South Africa | 1960 | .70 | 14.2 | *J* 1965 | 62.0 |
| Sudan | | | | *W* 1967-68 | 49.8 |
| Tanzania | 1972 | .45 | | *W* 1969 | 50.4 |
| Togo | 1970 | .52 | | | |
| Zaire | 1970 | .57 | 12.7 | | |
| Zambia | 1971 | .76 | 8.0 | *W* 1976 | 61.1 |
| Zimbabwe | | | | *J* 1968 | 69.1 |

TABLE A-1 (continued)

| | Land Concentration | | Agricultural Households without Land as % of Total Labor Force | Income Share of Upper 20% | |
|---|---|---|---|---|---|
| | Year | Gini | | Source and Year | % |
| Asia | | | | | |
| Australia | 1971 | .82 | 3.2 | *W* 1966–67, 1975–76 | 43.0 |
| India | 1971 | .62 | 38.1 | *J* 1968; *W* 1975–76 | 51.3 |
| Indonesia | 1973 | .56 | 22.5 | *J* 1971; *W* 1976 | 50.7 |
| Japan | 1970 | .42 | 1.4 | *W* 1969 | 41.0 |
| Korea, South | 1970 | .31 | 10.2 | *J* 1971; *W* 1976 | 44.4 |
| Malaysia | 1960 | .47 | 12.2 | *W* 1970, 1973 | 56.4 |
| Nepal | 1972 | .56 | | *W* 1976–77 | 59.2 |
| New Zealand | 1972 | .73 | 4.8 | *S* 1966 | 41.4 |
| Pakistan | 1972 | .51 | 32.4 | *J* 1971 | 41.5 |
| Philippines | 1971 | .51 | 35.6 | *W* 1970–71 | 54.0 |
| Singapore | 1973 | .30 | | | |
| Sri Lanka | 1973 | .67 | | *W* 1969–70 | 43.4 |
| Taiwan | 1960 | .46 | 5.0 | *W* 1971 | 39.2 |
| Thailand | 1963 | .46 | 20.0 | *W* 1975–76 | 49.8 |
| Vietnam, South | 1960 | .59 | | | |

## Concentration of Land

Data for 1966–75 are from the Food and Agriculture Organization (FAO) of the United Nations (1981). The Gini index of concentration is calculated using total areas of farms (in hectares) and standardized size categories. Data prior to 1966 are considered to be reasonable estimates of 1966–75. The 1964 Gini score for Guatemala is from Seligson et al. 1983; other pre-1966 data are from Table 4.14 of Taylor and Hudson (1972).

## Landlessness

These data, circa 1970, are derived from calculations by Prosterman and Riedinger (1982, tbl. 17) of the proportion of agricultural families that are owner-operators and collective or state farmers based on information from the 1960 and 1970 rounds of the FAO World Census of Agriculture and collateral estimates. Countries with 50% or more collective or state farmers are excluded. Agricultural households without land as a proportion of the total labor force are estimated by multiplying the proportion of agricultural families that are not owner-operators or state farmers by the percentage of the labor force employed in agriculture circa 1970 (from the data file of the *World Handbook of Political and Social Indicators*—see Taylor and Jodice 1983).

## Income Share of the Richest Quintile

The sources of the data are *A*, Ahluwalia (1976); *B*, Bornschier and Heintz (1979); *F*, Fei, Ranis, and Kuo (1979); *J*, Jain (1975); *L*, Leal (1983); *P*, Paukert (1973); *R*, Roberti (1974); *S*, Sawyer (1976); *T*, Trejos (1983); *US*, United States Agency for International Development (1983); *We*, Webb (1976); *W*, World Bank (1979–85). If more than one source is listed, the income share is the average value. Most data are for households, but in the interest of maximizing coverage, data for individuals are used in the cases of Barbados, Bolivia, Colombia, Gabon, Ghana, Hungary, Ivory Coast, South

Africa, Switzerland, Tunisia, and Zimbabwe. Data are not used if they pertain to economically active males only, to workers only, or otherwise are not comprehensive (e.g., based on tax statistics). The data for developed countries are posttax; data for less-developed countries are not adjusted for taxation but can be regarded as comparable to posttax data in developed countries (World Bank 1979, 185–86). Note that Iran is not included. The expenditure data analyzed by Jabbari (1981) and discussed in the text are not strictly comparable with the data in Table A-1; they probably underestimate the extent of income inequality that existed in Iran under the shah.

## Notes

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1. Midlarsky and Roberts distinguish between these cases in regard to the dynamics of coalition formation leading to different kinds of revolutionary movements. Although both countries had inegalitarian distributions of land, creating a potential for insurgency in each case, the revolutionary movement in El Salvador was more narrowly class-based than in Nicaragua, due to differences in population density that produced greater land scarcity in El Salvador than in Nicaragua. This difference is thought to have enhanced the likelihood of a successful revolution in Nicaragua.

2. Unless otherwise noted, data on land and income distribution referred to in the text are either from Table A-1 or, for years other than those in Table A-1, from the sources cited therein.

3. Based on a study reported by Cespedes (1979). Trejos (1983) reports the income share of the richest 20% of households in Costa Rica as 51.1% in 1971, 52.1% in 1974, and 53% in 1977.

4. These data are from Keddie 1968; see also Bharier 1971.

5. These and all subsequent data on civil and political liberties referred to in the text are calculated from the data file of the *World Handbook of Political and Social Indicators.* For a description of the ratings, see Taylor and Jodice 1983 (1:58–65).

6. The other three significant determinants of internal war, apart from lagged internal war, were current and lagged negative sanctions and a dummy variable for presence versus of a communist regime. The inhibitory effect of a communist regime is subsumed under our more general regime-repressiveness hypothesis.

7. The only previous research on this topic is reported in Midlarsky 1981, where income distribution is measured by an index of intersectoral inequality. As Sigelman and Simpson (1977, 111) have pointed out, however, this index "is at best a second-rate measurement proxy for personal income, lacking theoretical interest of its own."

8. Deaths from political violence are an attribute of political-protest events like riots, armed attacks, and assassinations. Deaths are thus a summary measure of the intensity of political-protest events. Deaths are used in preference to a composite index for the following reasons: (1) a single-variable indicator is more easily interpretable than a composite measure; (2) deaths will necessarily correlate very strongly with a composite measure such as that constructed by Hibbs (1973), which includes deaths, armed attacks, and assassinations; and (3) there is probably less reporting bias for deaths than for indicators such as armed attacks (see Weede 1981). Death *rate* is preferred over raw counts because the former is an indicator of the extent to which the regime is threatened by insurgency, which depends not on the absolute frequency of political violence but rather on its frequency relative to size of population (for further discussion of this issue see Linehan 1976; Muller 1985; and Weede 1981). The logarithmic transformation is theoretically appropriate because death rate from political violence is expected to vary as a positively accelerated function of inequality; it is also necessary because of the presence of extreme values—although the problem of extreme values still exists after logging. An increment of one is added to each death score before logging because the log of zero is undefined.

9. In testing the multivariate model across 62 cases, the following countries are missing data on intensity of separatism: Barbados, Gabon, Honduras, Ivory Coast, Malawi, Nepal, Sierra Leone, and Trinidad and Tobago. Based on country descriptions from Banks 1976, these countries were scored zero on intensity of separatism.

10. The number of cases is 83 because Ireland and South Vietnam are coded as missing on political violence for the 1973–77 period.

11. An alternate multiplicative model, which includes Gini land concentration, the size of the agricultural labor force, and the product of these variables, yields an $R_a^2$ of .06, a value that is identical to the $R_a^2$ for Gini land concentration alone.

12. An alternative operationalization of the landlessness concept is to express landlessness as a proportion of the agricultural labor force instead of as a proportion of the total labor force. Regression of the 1973–77 political-violence death rate on landlessness as a proportion of the agricultural labor force yields

an $R_a^2$ of .04, which is identical to that for landlessness as a proportion of the total labor force.

13. Because death rates are logged, the underlying model is monotonic but curvilinear, that is, unlogged death rate would plot as a positively accelerated function of income inequality.

14. Gabon, Brazil, Panama, and the United Kingdom are instances of deviant cases that depress the strength of the relationship between income inequality and political violence, while Zimbabwe is an instance of a case that enhances its strength because of an extreme score on the independent variable. The most influential case, as determined by Cook's D, is Gabon, followed by Zimbabwe, Brazil, the United Kingdom, and Panama. If Gabon is deleted, the regression coefficient for income inequality rises to .064, $R_a^2$ rises to .18, and the remaining most influential case is Brazil; if Brazil is then deleted, the income inequality coefficient rises to .072, $R_a^2$ rises to .22, and the remaining most influential case is Panama; if Panama is then deleted, the income inequality coefficient rises of .078, $R_a^2$ rises to .25, and the remaining most influential case is the United Kingdom; if the United Kingdom is then deleted, the income inequality coefficient rises to .084, $R_a^2$ rises to .29, and the remaining most influential case is now Pakistan (the sixth most influential case in the initial regression); if Pakistan is then deleted, the income inequality coefficient rises to .088, $R_a^2$ rises to .33, and the remaining most influential case is Zimbabwe; if Zimbabwe is then deleted, the income inequality coefficient declines to .082, $R_a^2$ declines to .28, and no further cases are influential.

15. One-tailed t-tests are used because the expected relationships are specified a priori in the theoretical model depicted in Figure 1.

16. There are 63 cases with information on income distribution. Of these, Ireland is coded as missing on political violence for both the 1968–72 and 1973–77 period, while Pakistan is coded as missing for the 1968–72 period. Information on energy consumption per capita is not available for Taiwan. Thus, depending on whether lagged violence and/or energy consumption per capita are included in a given equation, the N will vary between 60 and 62.

17. As predicted, the rate of negative sanctions is relatively independent of the structural repressiveness of the regime. The regime repressiveness variable correlates with the 1968–72 and 1973–77 negative sanctions variables at .26 and .21, respectively.

18. There are essentially two kinds of influential cases that could distort regression results: outliers (or extreme errors of prediction) and cases with extreme scores on the independent variables. Outliers can be diagnosed from the residuals, while Mahalanobis's distance is an index of the extent to which a particular case in a multivariate equation has unusual values of the independent variables. Cook's D is a useful summary measure of the extent to which a data point is influential due to being an outlier and/or having an extreme combination of scores on the independent variables. For a good nontechnical discussion of these diagnostics see Norusis 1986 (sec. B 207–13).

19. Weede (1986) has shown that Zimbabwe is an influential case in regard to the strength of the income-inequality effect across a smaller sample analyzed previously (see Muller 1985). Since Zimbabwe was influential principally because of an extremely high death rate from political violence, it was possible to take corrective action by setting a ceiling on the death rate, and Zimbabwe was no longer influential once this was done (Muller 1986). Zimbabwe also is not influential for this sample. It is not among the 10 highest values of Cook's D in regard either to Equation 1.2 or to 1.4; and when Zimbabwe is deleted from 1.2, the income-inequality parameter estimates do not change, while deletion of Zimbabwe from 1.4 also makes virtually no difference (the unstandardized regression coefficient is reduced from .034 to .032 and the standardized coefficient is reduced from .24 to .22).

20. We have not recalculated the upper 20% income shares for countries with average values derived from other sources in addition to the *World Development Report* because in every instance the other sources are either previous World Bank compilations (denoted *A* and *J*) or an Organization for Economic Development and Cooperation (OECD) compilation (denoted *S*) used in part as a source for data in the *World Development Report.*

21. The number of cases is 43 because Pakistan was not included in Equation 1.2 and also is one of the 18 deleted cases.

22. An asterisk denotes the presence of three or more cases.

23. Brazil and Panama are still deviant cases, however (Panama probably because of a high level of acts of governmental coercion, Brazil probably because of the presence of a semirepressive regime). They are the two data points in the lower-right-hand corner of the graph with income-inequality residuals slightly less than 2 and political violence residuals approximately equal to −1. Because other countries with low inequality residuals also have even lower political violence residuals than Brazil and Panama, heteroskedasticity is less of a problem.

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# POLITICAL CAPACITY AND THE DECLINE OF FERTILITY IN INDIA

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*With data from 15 Indian states, in this study I demonstrate that political capacity, defined as the ability of government to penetrate society and extract resources, has a more significant—though indirect—effect on fertility behavior than does level of economic development. A path-analysis model with six variables—crude birth rate (the dependent variable), family-planning-program effort, mean female age at marriage, physical-quality-of-life index (PQLI), income per capita, and political capacity—showed a strong indirect effect by the political-capacity measure on fertility decline through both the PQLI and family-planning effort. Within the model, income per capita had little effect on birth rate or any of the intervening variables. The conclusion drawn is that under conditions of economic backwardness as exist in India, it is politics, not economics, that is the primary determinant of fertility patterns.*

Social scientists investigating the dynamics of fertility behavior have paid relatively little attention to the political context. Explanations of demographic transition have emphasized economic development, quality of life—especially as it relates to women—the effect of such factors on the motivation to regulate births, and effective networks for the delivery of contraceptive information and services. When it is discussed at all, government has been considered only as the vehicle through which family-planning programs are delivered to the population and not as a significant causal variable. In contrast, using data from the Indian states, I propose to show that political capacity in terms of the ability of government to extract and distribute resources has a much greater effect, albeit indirectly, on the decline of fertility than the level of economic development under the conditions of relative economic backwardness that exist in much of the developing world.[1]

Most of the countries in these regions are "new nations" recently forged out of political struggles, where government has taken the key role of fostering economic growth and national integration. Given the low level of socioeconomic development in many of these nations, only a relatively weak economic infrastructure generally exists between government and the individual. As a result, government has emerged as the major actor influencing social and economic change. Under such conditions, one should expect that the greater the capacity of the political system, the more effective the implementation of social development policies, including family planning and such programs as health and education, which improve the basic quality of human life.

This article does not deny the significant relationship between economic development and decline in fertility demonstrated by numerous studies

(Cutright 1983; Easterlin 1975; Mauldin and Berelson 1978; Tabbarah 1971). Nevertheless, demographers and economists have been remiss for not including political variables, especially the performance capacity of government institutions, in their models of the determinants of fertility decline in poor nations like India. For the majority of the population in these countries, economic forces do not provide a strong rationale to regulate fertility and will not do so in the foreseeable future. In these circumstances, the political capacity to implement social development and family-planning policies, not the level of economic development, should become the major factor affecting birth rate.

In this study, I will test this proposition through statistical analysis of data from India's 15 largest states for the decade of the 1970s. With its great cultural diversity and wide regional differences in socioeconomic development, yet with a common constitutional system, India provides a rare and fascinating opportunity for comparative analysis of fertility. Measures of economic growth and quality of life vary widely among India's states. Although formally biased in favor of the Center (central government in New Delhi), the constitutional system is federal, and the state governments have significant powers. Many policy areas that have vital impact on the lives of individual citizens, such as health and family planning, come under the jurisdiction of the states. Despite the centralizing effect of national planning, implementation of national programs are heavily dependent on state-level administration and political leadership. As a result of this pattern of intergovernmental relations and of the wide differences in both political culture and the goals of the political elite across the states, there are sharp variations in the direction and emphasis of social-development policy at the state level and in the ability of the state governments to carry through on economic reforms.

## Theoretical Framework

Models of explaining fertility decline have grown in complexity and sophistication since the first formulations of a theory of demographic transition over three decades ago. Based largely on the historical experience of Europe and North America, the initial theory suggested a direct, sequential—if uneven—relationship between socioeconomic development and changes in mortality and fertility rates over time. As the modernization process—urbanization, industrialization, literacy, and the like—begins to take hold, first the death rate falls and, after a time lag, so does the birth rate. According to the theory, the benefits derived from increasing economic development, technological innovations, and the resulting higher standard of living raise the cost of children. Fertility declines because people make the rational decision that it is in their own economic self-interest and that of their children to have smaller families.

This initial model of demographic transition has proven to be a poor guide for population projections in the developing world (Teitelbaum 1975). More recent model builders focus on the proximate variables that intervene between environmental conditions and fertility behavior, the impact of family-planning-program effort, and the motivational factors that lead to reduced fertility. The primary emphasis is not demographic transition but the transition from natural to controlled fertility.

Bongaarts (1978, 1982) has shown that variation in total fertility rate (TFR)—the number of births a woman can be expected to have during her productive years—can be explained almost exclusively by four factors closely connected to fertility behavior. These factors, labeled the "proximate variables," are (1) the proportion married among females of reproductive age, (2) the proportion of fecund women using effective contraception, (3) the prevalence of induced abortion, and (4) the duration of postpartum infertility. With data from 41 populations—both developed and developing, as well as selected historical populations with available information—Bongaarts found that a model estimate of TFR derived from these four variables explained 96% of the variation in the observed fertility rate (1982, 184). While these variables most closely affect fertility, they, in turn, are influenced by other factors.

While drawing criticism over the years, family-planning-program efforts have begun to show an increasing impact on the decline of fertility in developing countries. Using an index of program effort derived by scoring 15 "programmatic criteria" for 94 countries in the early 1970s, Mauldin and Berelson (1978) found a close association with crude-birth-rate (CBR) decline for the period 1965-75. Family-planning-program effort affected fertility decline both independently and in combination with socioeconomic modernization. Alone, the index of program effort explained 78% of the variation in CBR decline while the multiple $R^2$ with seven socioeconomic variables explained 83% of the variation.[2] More recently, Lapham and Mauldin (1984, 1985) have revised and expanded the index to include 30 items and gathered information for 1982 on 93 countries. In a path model with contraceptive prevalence as the dependent variable, socioeconomic setting had a slightly greater effect with a .53 beta coefficient compared to the index of program effort with a beta of .46. While the total effect of socioeconomic setting was greater, the correlation coefficient of program effort with contraceptive prevalence reached .89 (1985, 127).

The third major focus of the study of fertility decline has been on motivational factors that lead to a shift from natural fertility to deliberately controlled fertility (Bulatao and Lee 1983, vol. 1, 1983, vol. 2; and Easterlin 1975, 1978). This research is concerned with pinpointing those conditions of socioeconomic modernization that result in efforts by individual couples to deliberately limit family size and thus to lower fertility. According to this theory of fertility transition, at lower levels of development, natural fertility tends to be the norm. Most married couples make no effort to regulate their fertility since they generally are unable to have as many children as they desire. Reasons such as fewer surviving children, poor nutrition and medical services, and extended lactation practices result in a situation where potential family size is less than desired family size. The demand for children exceeds the supply. There is no motivation to limit fertility, and so the typical couple's rational response to these circumstances becomes natural fertility. Supporting this behavior is the generally limited knowledge of birth control methods and the high cost of regulation. But as socioeconomic modernization progresses alongside a continued absence of deliberate regulation, the supply of children begins to exceed the demand for children. Increased education, urbanization, and improved economic conditions raise the cost of children, reducing the desired family size. At the same time, potential family size begins to rise as a result of higher child survival rates and higher natural fertility resulting from improved public health programs and better nutrition. Educational expansion and increased knowledge and acceptance of the techniques of fertility regulation reduce the cost of fertility control. From this combination of changes emerges the motivation to reduce fertility, which in turn leads to deliberately controlled fertility and a decline in birth rate.

For explanatory models, the question turns into one of discovering those factors in socioeconomic development most likely to generate the motivation to control fertility deliberately. The initial demographic transition models considered economic growth and industrialization to be the key variables in the process (Cutright 1983). But only a few Third World countries will attain a level of industrial development where the economic conditions of the mass of the population can be expected to improve dramatically in the foreseeable future. In addition, such indicators do not take into consideration the distributional pattern of development on populations at low levels of economic development. It has become increasingly clear that changes in gross national wealth or industrialization have very little effect on fertility behavior (Caldwell 1976; Cassen 1978; Freedman 1979).

Motivation to reduce family size seems to be most closely related to development strategies that benefit the poor majority. In those developing countries where fertility has dropped, motivation for family limitation has been generated by a heightened quality of life, especially for women. Where there are increased educational opportunities for the poor as well as the rich and for women as well as men; where women have rights beyond the traditional extended family and have some capacity to manipulate their personal environment; and where governments increasingly assume welfare and social-security functions traditionally fulfilled by the family—fertility rates decline in response. This dimension of development has been most frequently measured by indicators of education and health. Such measures are more likely to gauge improvements in the conditions of the lives of the poor through broader distribution of societal wealth. Most studies have found adult literacy rate, especially female literacy, to be the most significant indicators of educational development. Health conditions of populations are most commonly measured by nutrition intake, life expectancies, and infant mortality rate. In particular, a decreasing infant mortality rate may have a direct positive effect on motivation to limit family size, as well as lower the birth rate by prolonging infecundity due to breast-feeding. The status of women has also been directly linked to motivation to reduce fertility (Cain 1984; Safilios-Rothschild 1982). In societies where women are basically under the control of a patriarchal extended family, where they are generally excluded from the mainstream of economic activities, and where sons are the primary means of security, high reproduction tends to be the source of status. In societies where women are relatively independent economically, where they have relative equality with men to participate in the nonagricultural work force, and where sons are not placed at a premium, women have more autonomy over their own lives and greater motivation to limit the number of children they bear.

If improved family-planning-program effort, greater motivation to reduce family size due to improved quality of life, and higher female status do in fact reduce fertility rate, what societal forces in turn influence these changes? What complex of factors lies behind the successful implementation of a family-planning program? How can the quality of life be improved substantially even though poverty and economic stagnation remain relatively unchanged? How can the status of women be radically changed without widespread modernization of the society? It is precisely in regard to these concerns that the political system must be considered in any theoretical model of fertility transition. Politics should have a significant, if only indirect, effect on fertility behavior in at least three places where causal connections have been drawn: improvements in the quality of life, improvements of the rights of women, and the effectiveness of the family-planning-program infrastructure. In the absence of rapid and sustained economic growth, improvements on these three fronts can be achieved only as a result of positive decisions on the part of the political elite to initiate such policies and only with government institutions capable of implementation. Without a high level of political capacity, even the best intentions of leadership to instigate programs of economic reform, emancipation of women, and family planning will make very little progress.

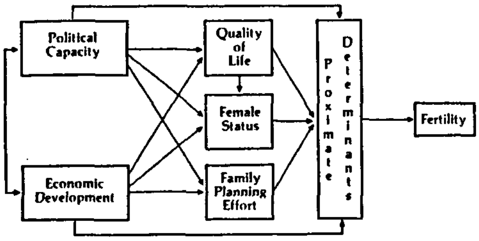
Political capacity refers to government's ability to penetrate society to secure compliance with its policies and to extract and distribute resources (Kugler et al. 1983; La Palombara 1971; Organski et al. 1984). The concept also implies the existence of institutionalized organizations, both governmental and nongovernmental, that contribute to mobilization of popular participation and facilitate elite-mass communication and cooperation over policy issues, which in turn contribute to more effective penetration. The concept implies not the personal capacities and preference of individual political leaders but the ability of the political system to perform as a totality. Of course, some political leaders will have greater ability and will to succeed than others. But, more importantly, governments perform better at extracting resources and achieving compliance not because their political leaders have more determination but because the political structures in which the leaders operate permit them to act in such a way.

Acceptance of policies and availability of resources permit government to carry out tasks expected of it. In poor countries, a high level of political capacity can make it possible for a redistribution of societal goods and benefits on a relatively equitable basis, if that happens to be the goal of the prevailing political elite. Where sufficient political capacity does not exist to extract resources and enforce redistributive policies, economic forces generally work in such a way that benefits accrue primarily to those groups that are already the most affluent. In such situations, the quality of life of the mass of the population does not improve, and there is little motivation for people to change existing patterns of fertility behavior.

Finding a reliable measure of political capacity with validity across nations has proved difficult. Few political scientists have ventured beyond abstract conceptual definitions of the phenomenon, resulting in a variety of interpretations. The most frequently used measures have either been based on configurations of the electoral and party systems or have been closely linked to economic productivity. But reliable indicators of political capacity must measure the performance of the government itself, not the performance of a surrogate like the party system. In a significant break with earlier patterns, Organski and his collaborators suggest the tax structure as the best measure of the overall capacity of the political system (Organski and Kugler 1980, chap. 2; Organski et al. 1984, 62-67). The imposition of taxes is perhaps the most direct indicator of government's ability to extract society's resources. Few operations of government have greater impact on the lives of individuals than do taxes. None is more crucial in determining what groups in a society benefit most from its wealth and which are penalized. Without revenue, there can be no preservation of order, no redistribution of resources, and, indeed, no government.

But economics as well as politics define the parameters of government extraction. The gross amount of revenue raised depends at least as much on the wealth of a society as on the ability of the political system to collect resources. A valid indicator of political capacity must minimize the effects of economic conditions on tax effort. One strategy for accomplishing this is to calculate the ratio of a political entity's per capita tax revenue to its per capita GNP. Even with this procedure, one cannot be certain that the presence of economic factors has been eliminated. But the measure controls for level of wealth and yields an indicator with only a

**Figure 1. Analytical Framework for Assessing the Relationship of the Political System to Fertility Decline**



modicum of contamination by the influence of the economic system.

These theoretical formulations suggest the basis for a model of the relationship of the capability of political systems to fertility behavior in developing countries. Complex flow models show a variety of determinants from socioeconomic conditions to proximate variables influencing fertility outcomes. The present model does not attempt to take all these components into account but instead focuses attention only on those factors that appear to be related theoretically both to fertility decline and the capacity of the political system. Figure 1 incorporates six variables: political capacity, economic development (or wealth), quality of life, female status, family-planning-program effort, and fertility decline. The specific indicators of these variables in the Indian context will be discussed below. The proximate determinants are not included in the statistical analysis. Rather the model assumes that the effect of the five independent variables on fertility decline must go through these more immediate variables. One-way arrows indicate the causal paths from each determining variable to the variable dependent on it. Variables in the model presumed not to be causally connected are connected by two-headed arrows. The model also assumes both level of economic development and political capacity to be exogenous variables, that is to say, it assumes they are caused by variables outside the set under consideration and have no causal relationship to each other. While these two variables may often be found to be statistically associated, the direction of dependence is in question.

The significance of the model is in the illustration of how political capacity and fertility decline are connected via quality of life, female status, and family-planning-program effort. The model tests the hypothesis that the capacity of the political system measured in terms of tax effort directly influences improvements in the quality of life, the status of women, and the ability to deliver family-planning services and that these in turn have a major impact on the decline of fertility through the proximate variables. The level of economic development is also included in the model because it is so often presumed to directly influence these three intermediate factors as well as have a direct effect on fertility. The analysis carried out here also tests the assertion that in an environment where the level of economic development is low (the Indian states fit this condition), the capacity of government is a more significant factor influencing fertility decline than the gross wealth of the society. In other words, the indirect effects of political capacity will outweigh the combined direct and indirect effects of economic development.

## The Model Applied to India

India has one of the most severe population problems in the world today. The 1981 Census of India recorded a population of over 690 million, second only to that of China's estimated 985 million. With a current annual growth rate of 2.23%, or 15 million additional people each year, India's population is expected to approach or exceed one billion by the year 2000. India falls in the middle of the demographic transition cycle. Since the

1940s; with improved control over famine and many infectious diseases, mortality has fallen substantially. In 1941 India's crude death rate was estimated at 31.2 per thousand, but by 1978 sample registration system (SRS) data showed the crude death rate to have fallen to 14.5. In contrast, the CBR dropped from over 45 per thousand in 1941 to 34 per thousand by 1978. As a result, India's population grew nearly 25% in each of the two decades prior to the 1981 census.[3]

Over the same twenty-year period, India's fertility rate declined about 25% from a TFR of about 6.5 prior to 1950 to an estimated 4.8 in 1981. Indeed, India's fertility rate is lower than any other country at a similar or lower level of socioeconomic development. Among the individual Indian states, wide variations exist. Based on SRS data, five states have CBRs under 35 while in four others the rate remains 37 or above. In this analysis, state-level fertility data has been taken from estimates made by the National Academy of Sciences Panel on India and reported in Preston and Bhat 1984. From 1981 census sample age distributions and from 1972

**Table 1. Independent Variables in the Model of Family-Planning-Policy Performance among the Indian States**

| State | Political Capability (Per Capita Tax Revenue as Percentage of per Capita Income, 1975)[a] | Income per Capita (Average 1973–75)[b] | Physical Quality of Life Index (1970–75)[c] | Mean Age at Marriage (1971)[d] | Rural FP Personnel per 10,000 Rural Population (1975)[e] | Crude Birth Rate 1971–81 Based on Intercensal Population Change[f] |
|---|---|---|---|---|---|---|
| Andhra Pradesh | 7.8 | 928 | 43 | 16.4 | 22.4 | 33.2 |
| Assam | 3.6 | 791 | 37 | 18.4 | 5.7 | 40.5 |
| Bihar | 4.9 | 645 | 31 | 15.5 | 9.2 | 39.1 |
| Gujarat | 9.4 | 1,131 | 40 | 18.3 | 20.9 | 36.4 |
| Haryana | 9.4 | 1,399 | 42 | 18.8 | 22.3 | 37.1 |
| Karnataka | 10.2 | 1,045 | 48 | 17.9 | 15.0 | 33.1 |
| Kerala | 10.7 | 948 | 70 | 20.9 | 22.7 | 28.1 |
| Madhya Pradesh | 6.5 | 776 | 37 | 15.2 | 21.7 | 36.4 |
| Maharashtra | 9.7 | 1,349 | 49 | 17.5 | 17.5 | 31.2 |
| Orissa | 5.0 | 793 | 37 | 17.2 | 15.2 | 33.2 |
| Punjab | 9.0 | 1,586 | 50 | 18.8 | 21.2 | 31.7 |
| Rajasthan | 6.5 | 778 | 33 | 15.4 | 10.7 | 40.9 |
| Tamil Nadu | 8.3 | 942 | 46 | 19.6 | 23.4 | 30.2 |
| Uttar Pradesh | 5.0 | 715 | 24 | 15.6 | 13.0 | 40.4 |
| West Bengal | 6.4 | 1,033 | 42 | 17.8 | 13.9 | 37.5 |

*Sources:*

[a] Jain (1978, 67).

[b] Centre for Monitoring Indian Economy, *Basic Statistics Relating to the Indian Economy,* vol. 2, *States* (1982, tbl. 14.1).

[c] India, Department of Family Welfare, *Family Welfare Programme in India Yearbook, 1977–1978* (1979, 36, 49, 63).

[d] India, Department of Family Welfare, *Family Welfare Programme in India Yearbook, 1975–76* (1977, 21–22).

[e] India, Department of Family Welfare, *Family Welfare Programme in India Yearbook, 1975–76* (1977, 111).

[f] Preston and Bhat (1984, 500).

Note: The CBR figure for Assam is an interpolation made by the author from the all-India estimate in Preston and Bhat 1984 and the unadjusted SRS estimate of CBR for that state in the 1970s.

and 1979 surveys in SRS sample areas, the authors of the report estimated 1971-81 intercensal CBRs for 14 Indian states. The statewide figures for this and other variables included in the model can be found in Table 1.

Few countries in the Third World have invested more effort or expenditure on a family-planning program than India. In 1952, India became the first nation in the world to adopt family planning as an integral part of its national-development planning; but the program has never achieved the goals of the development planners, and over years it has been subject to much criticism (Ledbetter 1984; Mamdani 1972). After a modest beginning, the Indian government gave the program increased emphasis following the 1961 census indicating a sharp jump in population over the previous decade. A new "cafeteria approach" was introduced, making available all modern clinical and conventional contraceptive methods through family-planning clinics. Combined with this new approach was an extensive media campaign to convey the advantages of small families. The 1970s began with a high level of optimism, and Indian family planners predicted a fall in CBR to 30 or below, with a 33% increase in the proportion of couples effectively protected in the reproductives ages (Visaria and Jain 1976, 41). By 1972, total accepters had reached over five million, whereas 10 years before, there had been fewer than 200 thousand. The greatest emphasis in the program was now placed on male sterilization, and, with mass vasectomy camps spreading around the country, the program appeared on the verge of a significant breakthrough. But after 1973, the number of sterilizations fell because of administrative problems, financial constraints, and adverse rumors about sterilization's effect on male potency.

The Emergency (June 1975-January 1977) saw renewed intensity injected into the program. The new population policy of April 1976 advocated disincentives and compulsory sterilization under certain circumstances. But implementation of the program led to widespread coercion and abuses, especially in the Hindi-speaking northern heartland (Paipanandiker, Bishnoi, and Sharma 1978). When the Janata party came to power following the 1977 elections, largely as a result of popular revulsion against the program, all coercive pressures were dropped and the name of the program itself was changed to "family welfare" to remove the stigma of the Emergency. Even so, a widespread loss of faith in the program and demoralization of family-planning personnel set in, greatly reducing the acceptance rate. In 1981, the proportion of couples effectively protected was only 23.7%, far short of the goal set a decade earlier and no greater than the percentage effectively protected for 1976-77 (India, Department of Family Welfare 1983, 107).

A number of possibilities exist as indicators of family-planning effort. Unfortunately, there is no such measure for the Indian states as elaborate as the Lapham and Mauldin (1984) cross-national measure of family-program effort. In India, the Center allocates the family-planning budget, most revenues expended come from this source, and important policy decisions on alternate programs are made there. States are responsible for implementation of the program, and they may enact laws on family planning and appropriate revenues for the program in addition to those coming from New Delhi. By the 1970s, the primary unit for delivering family-planning services in rural areas was the Primary Health Center (PHC). Ideally, each PHC was intended to cover all or most of an administrative "block" (India is divided into about 55 hundred blocks), have two doctors (one, ideally, a woman), and a support staff (nurse-midwives, health assistants, and others) of one staff member per 20 thousand people. Extending from the PHCs, the program authorized subcenters staffed by a nurse-midwife and at least one health attendant on a ratio of one subcenter per 10 thousand population (Cassen 1978, 147). But by the time of the establishment of the Emergency in June 1975, no state had built or staffed all the PHCs and subcenters authorized to disperse family-planning services. Among the states, a wide variation existed in the amount of infrastructure completed. As a result of this differential, the number of family-planning staff personnel in rural health centers and subcenters per 10 thousand rural population in 1975 may be taken as a reliable indicator of a state's ability to supply services and of family-planning effort.

In this study, the level of economic development in the Indian states has been measured in terms of income per capita. The actual measure is the three-year average for years 1973-1975. Because of the different impact of the vagaries of the monsoon on agricultural production in several states over the years, mean scores were considered more reliable than figures for a specific year. The quality of life of a given population can be measured in a variety of ways, as suggested above. The key property of such measures is that they should identify conditions that effect the well-being of the very poorest people. The physical-quality-of-life index (PQLI) developed by Morris David Morris (1979) combines three of the most important indicators of basic human needs: infant mortality, life expectancy at age one, and literacy rate. Giving equal weight to each variable, the index provides a summary measure of how well a society meets basic needs of its poorest citizens. In this sense, it is a better indicator of overall improvements in quality of life than infant mortality rate used alone (the most common measure of this dimension in studies of the determinants of fertility decline) and is used in the present analysis as the measure of quality of life in the Indian states. Like the previous indicators, the data cover the first half of the 1970s.[4]

Under social arrangements as exist in India, where nearly all women of marriagable age are married and where women are often constricted by marriage, the average female age at marriage can be taken as a prime indicator of the degree of female autonomy.[5] In India, women—and men as well—have historically married very young. According to the 1921 census, the average age at marriage for women was 13.9 years rising only to 15.8 years in 1961. By the 1971 census (the figures used in this study), it had reached 17.1 years. Generally speaking, the lower the average female age at marriage, the lower the level of personal autonomy of women. Where young adult women have few rights in the familial environment and virtually no control of the choice of partner, their only purpose in life becomes marriage and the production of children, especially sons—which fosters early marriage. Early marriage also has economic advantages because of marriage cost for the family of the bride. Dowry requirements generally increase with the age of the daughter (Dyson and Moore 1983, 48). In this perspective, early marriage would seem to indicate stronger pronatalist pressure on women as well as expand the years of possible female fertility.

The tax structure in India allows for the construction of a measure of political capacity in accordance with theoretical considerations discussed in the previous section. The constitution of India divides taxing authority between the Union and the states. Although the Union's tax resources are considerably greater than those of the states, the taxing powers of the states are not insignificant nor are they narrowly defined. States' taxing authority falls into three categories: taxes on commodities and services, including the general and specific sales taxes; taxes on properties and capital transactions, such as urban property tax and registration fees; and taxes on agricultural income and on professional employment. In each of these areas, individual states may or may not levy taxes, and there is potential for growth, especially on agricultural income, which most states have so far been reluctant to tax. As a result of these optional powers, the structure and the amount of taxation varies substantially among the states. Revenues from the Center are allocated to the states through the national-planning process on the basis of recommendations from the independent Finance Commission. These financial transfers take the form of shared taxes, grants-in-aid, and loans. While redressing regional imbalances has been a factor in the allocation decisions, the dominant consideration has been population equity. But because states have the option to tax on their own, the proportion of state revenue coming from the Center varies considerably from a low around 40% to almost 95% for some of the small states of the northeast (Cutt 1969; Jain 1978; Sharma 1974).

The extent to which states rely on their own tax effort provides the basis of an index of political capacity for the Indian states. Because a state's tax effort generates revenues in addition to those distributed by the Center, the amount of revenue raised by a state government through its own tax effort suggests the degree of its ability to penetrate society and extract resources independently of the central government. Using total state revenues (state and Union) as the bases of the indicator, while providing a more proper profile of a state's entire resource base, would not accurately indicate individual state taxing capacity, which is the primary requirement of the measure. As suggested above, adjustments must be made in the index to minimize the effect of overall state wealth on the amount of taxes the government generates. In India, richer states raise more revenue and require less transfer of central funds than poorer states. In order to control for wealth, per capita state tax revenue must be taken as a percentage of state per capita income. The resulting measure of the state's "tax sacrifice" is used in this study as the indicator of its political capacity.[6]

## Analysis

Recent analysis of fertility decline in the Indian states demonstrates the importance of improvements in quality of life, especially infant mortality rate and adult female literacy, for generating motivation to regulate fertility (Jain 1985; Jejeebhoy 1984; Srinivasan, Jejeebhoy, Easterlin, and Crimmins 1984; and Zachariah and Patel 1984). Improvements in family-planning services, while not insignificant, seem to account for a lesser proportion of variation in state-level fertility rate. Data examined by Zachariah and Patel showed that 91% of variation in TFR for 1978 was accounted for by socioeconomic variables, most importantly female literacy (1984, 31). Jejeebhoy's analysis of survey data from 10 states for 1970 (Operations Research Group, 1970) indicated that decline in infant mortality rate was the major factor related to the shift from natural to controlled fertility. She concluded that the success of a family-planning program in early transitional societies such as India's "is largely a function of improvements in infant and child health" (1984, 197). Jain found that contraceptive use, female age at marriage, and infant mortality rate explained about 90% of the variation in state-level birth rate in 1981. In his model, the effect of female literacy was transmitted through these three factors, mainly infant mortality. In contrast, levels of poverty show no relationship to variation in birth rate (1985, 192-95).

These studies generally confirm that

Table 2. Bivariate Correlations for Variables Included in the Model

| Correlation Matrix | Crude Birth Rate | Rural FP Staff | Age at Marriage | PQLI | Income per Capita | Political Capacity |
|---|---|---|---|---|---|---|
| Crude birth rate (CBR) | 1.00 | - .70 | - .64 | - .84 | - .44 | - .70 |
| Rural FP staff | | 1.00 | .42 | .56 | .51 | .72 |
| Age at marriage | | | 1.00 | .80 | .51 | .58 |
| Physical quality of life index (PQLI) | | | | 1.00 | .49 | .77 |
| Income per capita | | | | | 1.00 | .69 |
| Political capacity | | | | | | 1.00 |

$R^2 = 79.0$ F-ratio = 6.92 $p < .01$

part of the theoretical perspective discussed above that states that in societies at low levels of development, improvements in basic quality of life are more significant to fertility behavior than gross societal wealth. What they do not address is the question of how quality of life may be improved without the impetus of substantial economic growth. My analysis confirms these previous findings and demonstrates the importance of political-system capability for improvements in quality of life and family-planning-program effort. The analysis is in two parts: an examination of measures of association among the variables, followed by an application of the path-analysis technique.

## Correlation Analysis

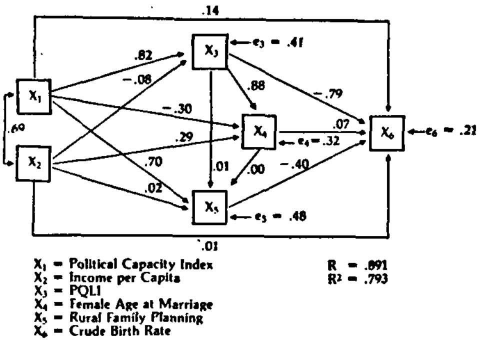
Table 2 gives the correlation matrix of the variables included in the model and the multiple $R^2$ of the independent variables with CBR 1971-81. Looking first at the correlations between the independent variables and the dependent variable, all are in the predicted direction and, with exception of income per capita, are strongly associated with CBR. The coefficient of multiple determination ($R^2$) demonstrates that together the independent variables explain 79% of the variation in state-level fertility rates. The PQLI, which incorporates infant mortality rate, life expectancy at age one, and literacy rate, shows the strongest relationship with fewer births. Alone, it accounts for 71% of the difference between the states. The correlation between political capacity and CBR is also high with $r = -.70$, indicating the possibility of some direct connection. The weakest of the simple correlations is that of income per capita with CBR. These results, while requiring the support of more complex statistical analysis, support the previous studies cited above, giving further credence to the argument that in societies with low levels of economic development such as India, meeting basic human needs will have greater impact on fertility behavior than the gross wealth of the society.

More important to the theoretical thrust of the present study, the correlations displayed in Table 2 demonstrate the close association between political capacity and the variables hypothesized as intervening between it and fertility behavior. With each of these variables—the PQLI, rural family planning staff, and age at marriage—the political capacity index has a stronger association than does income per capita. Even without further evidence, these results indicate that the capability of the political system, operationalized in terms of tax effort, is closely related to public policies that improve the quality of life of the poor and provide a more effective family-planning-program effort. Clearly, these data support the assertion that differences in government effectiveness are more important than differences in level of economic development, at least at low levels of development, when it comes to implementing societal conditions that increase motivation to reduce family size and that reduce the cost of fertility regulation.

## Path Analysis

Path analysis is essentially a method of decomposing and interpreting the direct and indirect causal relationships among a set of variables through a systematic application of multiple regression procedures. It allows one to go beyond associative reasoning and helps clarify causal thinking. Like multiple regression, path analysis furnishes an overall estimate of the explanatory value of a model. It allows the testing of a theoretical model by helping to identify the spurious and valid relationships in the determination of the dependent variable through the use of empirical data. In short, path analysis gives the capacity to base causal thinking on a more solid empirical foundation (Duncan 1966; Dye and Pollack 1973; Land 1969).

Figure 2. Model Depicting All 15 Path Coefficients Among the Six Variables



The hypothesized model set forth in the first section predicted seven significant relationships between the six variables: political capacity with (1) quality of life, (2) age at marriage, and (3) family-planning effort; and fertility decline with (4) income per capita, (5) quality of life, (6) age at marriage, and (7) family-planning effort. The next step in testing the model involves computing a set of simultaneous equations in which the dependent variable is first regressed against all the independent variables in the model, and then each succeeding intermediate variable is treated as the dependent variable. These six separate multiple regression computations yield 15 partial standardized regression coefficients or beta coefficients, which can be interpreted as measuring the direct relationship between two variables, holding the other variables in the equation constant.[7] The paths from the residual factor to each sequential variable held as dependent is calculated as $1 - R^2$, that is to say, the square root of the unexplained variance, which is sometimes referred to as the *error term.* The political capacity index and income per capita in this model are considered to be exogenous variables, meaning that they are assumed to be explained by factors outside the model.

Figure 2 displays the complete set of 15 path coefficients. As suggested from the correlation analysis, a number of predicted causal paths are weak. The path coefficients to CBR from political capacity, income per capita, and age at marriage do not meet the criterion of significance set at a coefficient of .20. In the case of political capacity, this was expected, as its effect on the decline of fertility was predicted to be primarily indirect. The high direct correlation seems to be accounted for by other variables in the model that more directly impinge on fertility behavior. In contrast, many previous studies have suggested that economic modernization and industrial development are significant variables in demographic transition. But here the effect of income per capita completely disappears when included in the multiple regression equation with measures of quality of life and family-planning effort.[8] This result gives strong support for the proposition that at low levels of economic development, differences in aggregate wealth are less important than differences in meeting basic human needs when it comes to creating motivation to reduce family size.[9]

The disappearance of the strong relationship between age at marriage and CBR appears to conflict with recent studies by Jain (1985) and Zachariah (1984), which have suggested that increasing age at marriage accounts for a substantial portion of fertility decline in India. But in the present regression model the observed high correlation coefficient between female age at marriage and CBR appears to be the product of variation in the PQLI. The path coefficients from the PQLI to CBR and age at marriage, respectively, are −.79 and .88. This seems to indicate that both increasing female age at marriage and decline in CBR are mainly the result of improvement in quality of life. While female autonomy may be clearly related to fertility decline, more basic are the growth in adult-female-literacy and child-survival factors captured by the PQLI. It appears, therefore, that creating better health conditions and educational systems are more important to fertility transition in India than raising female age at marriage.

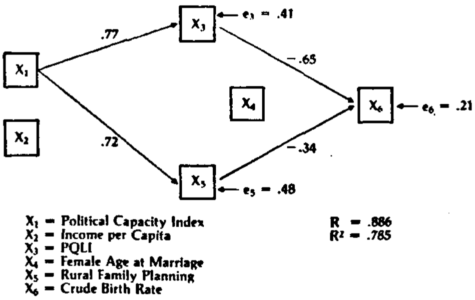
The most significant finding illustrated in Figure 2 from the point of view of this study is the discrimination of the role of the political system as an indirect influence on fertility behavior through the factors of quality of life and family-planning-program input. In the six-variable path model, strong relationships between the political-capacity index and both the PQLI and program effort are clearly demonstrated. In contrast, the path coefficient between the political-capacity index and age at marriage is low and not in the predicted direction. Finally, Figure 2 shows that income per capita is not significantly associated with the PQLI or family-planning effort when the political-capacity index is included in the same regression equations.

Although the path model depicted in Figure 2 accurately reflects the relationship between the six variables, it is unwieldy because of weak relationships between certain of the variables. One feature commending path analysis is that it facilitates the search for more parsimonious models with high explanatory power. While the elimination of paths does result in some distortion of reality, it is theoretically superior to explain the most variation in the dependent variable with the fewest possible variables. Paths are eliminated from the model if the standard error of the beta value in the regression equation from the independent to the dependent variable is greater than the beta value itself. Simply put, the standard error of beta tests the significance of the relationship between the variables.

This procedure leads to the elimination of seven paths: p61, p62, p52, p32, p53, p64, and p54. By dropping the paths from the age-at-marriage variable to both CBR and family-planning effort, the former becomes superfluous to the explanation of the dependent variable and can be dropped from the model. With the exclusion of this variable from the model, three other paths become redundant—p43, p42, and p41—which in turn makes it unnecessary to include income per capita as a variable in the model as well. What remains is a simple four-variable path model.

These simplifications yield a new set of simultaneous equations and the accompanying causal model depicted in Figure 3. Despite the loss of two variables and all but four paths, the model appears to have lost little of its explanatory power. The $R^2$

**Figure 3. Model Depicting Only Significant Paths That Directly or Indirectly Effect Crude Birth Rate**



for the four-variable model at 78.5 is essentially the same as that of the original model. Nor has there been any perceptible loss in the explanation of the two intermediate variables by omitting income per capita from the model. In neither case did the amounts of unexplained variance increase. The new parsimonious model gives a clearer picture of the relationship between the capacity of the political system and fertility decline.

First, the new model indicates that, in a three-variable multiple regression equation, the PQLI explains about three-and-a-half times as much variance in the CBR as the measure of family-planning effort.[10] Because the three other hypothesized predictor variables contribute little directly to the explanation, this estimate adds a more precise dimension to the enucleation of the determination of fertility behavior in India. It can be stated with even more certainty that under conditions of low economic development, such as exist across the states of India, the primary factor contributing to a decline in birth rate is not relative differences in societal wealth but improvements in basic human needs. No doubt the economic wealth of a society becomes a major factor effecting motivation to reduce birth at higher levels of economic development, as many studies suggest. What the current findings indicate is that a threshold of economic development must be reached before this effect can take place. Below this threshold where the Indian states lie, as do most of the countries in Africa and Asia, the motivation to curb fertility is strongest where there have been improvements in health conditions and adult literacy as a result of the social policies of government, regardless of economic development level.

The results illustrated in Figure 3 demonstrate that family-planning-program effort, while having a lesser effect than improvements in quality of life, has also had a significant impact on the reduction of birth rate in India despite the program's drawbacks and problems over the years. This finding generally supports the results of recent state-level studies of determinants of fertility decline in India cited above, which suggests that once motivation to reduce birth rate has begun to develop, through improvements in the quality of life of the mass of the population, effective delivery of family-planning services contributes significantly to the reduction of CBR within a state. Program effort, therefore, appears to be a necessary but not a sufficient condition for fertility regulation in India.

Effective family programs, as with effective health and social-development programs are, to a large extent, the product of capable government institutions, which must originate and administer them. The path-analysis results depicted in Figure 3 make this point quite strongly. The political-capacity index explains over 50% of the variation in both the PQLI and the program-effort measure. The elimination of income per capita from the model does not weaken the explanation of these intermediate variables. When effect coefficients are calculated for the model, it can be seen that the indirect effect of political capacity on birth rate through these variables is greater than the direct correlation between income per capita and birth rate.

Effect coefficients provide a measure of the direct and indirect effects of a variable within a model. The indirect effect is that part of the total effect of a variable transmitted by variables specified as intervening between an independent and dependent variable in a model and is measured by the sum of the joint association between the variables (Alwin and Hauser 1975, 39). Because the direct path between the political-capacity index and CBR has been omitted from the new model, the effect coefficient measures only the indirect effects. If we use the formula $E = p31 (p63) + p51 (p65)$, the effect coefficient comes to $-.75$. This means that 56% of the variation in CBR can be explained indirectly by the political-capacity index through the intervention of the PQLI and family-planning effort. No claim is being made here that this figure is a precise measure of the degree to which governmental capacity influences fertility behavior in India, but it should be taken as solid evidence that the political system does effect the networks that structure fertility decisions and that this effect is greater than that of aggregate economic wealth.

The findings described in this article clearly suggest the need for opening a new dimension of demographic research. Among the Indian states, political development, as defined here, is a more important determinant of the decline in fertility than economic development. While there may be some direct effect (for example, through the establishment of a more secure and predictable political environment), the primary effect appears to be indirect. Capable government institutions are able to implement, with relative effectiveness, social-development and family-planning programs that create an environment in which couples develop the motivation to reduce the number of child births and have the services to allow them to do so. Economists and demographers should not overlook this role of the political system in affecting fertility patterns in the poor nations of the Third World.

## Conclusion

This study, while more limited in its regional scope, goes beyond the important and original work of A. F. K. Organski and his collaborators (Kugler et al. 1983; Organski et al. 1984) in its theoretical implications. While the Organski group first demonstrated that capable government had a significant effect on change in vital rates, the findings of this study indicate that the primary effects of the political system on fertility behavior are indirect though effective implementation of social policy. Additionally, the data presented here show that family-planning programs do make a difference when linked to improvements in basic human needs and that the effectiveness of such programs in turn are directly connected to political-system capability. But the primary argument of this study remains the same, namely, that, under conditions of economic backwardness, it will be politics, not economics, that can make the difference on the fundamental issue of overpopulation. Where the economic system is not highly differentiated and poverty is prevalent, the political system becomes a more important agent effecting human well-being. Policy planners and students of population change in the Third World ought to pay more attention to this fact.

The disclosures of this study indicate some distinct policy directions that, if taken by low-income countries such as India, should facilitate the shift from natural to controlled fertility. In India, the combination of public policies that lead to basic improvements in quality of life—such as those aimed at universal primary education and access to minimum health care for all citizens—and public policies that increase availability of family-planning services appears to be the key to changing the fertility behavior of the poor. As the data analysis suggests, social-development programs are more important than the family-planning program. But both are necessary. Once motivation develops out of improved living conditions, the means of regulation must be easily accessible to and affordable by the poor. The bottom-line inference from this study is the need for the institution of a basic family-security program, one that involves not only family-planning services and basic health care but begins to provide unemployment and old-age security, which in developing societies have traditionally been furnished by the family. The typical response to such a suggestion is that India and other overpopulated low-income countries are too poor to develop such programs. But this reply, in turn, brings the argument back to the role that can be played by the political system.

Countries at the low end of the economic-development scale can provide basic family-security programs if governments have the capacity to extract scarce resources and the social goal to distribute these resources in a more equitable manner. Extractive capacity is the most crucial. The best of political intentions cannot succeed without effective government. Exactly how government acquires this capacity is beyond the scope of this essay, but it most certainly involves popular involvement in the political process, which both generates demands on government and gives government the legitimacy to make and enforce difficult decisions. In short, with regard to fertility transition, where there exists what Gunnar Myrdal has labeled a "soft state," in the unlikely event of rapid and significant economic growth, population expansion will go unchecked, reaping continued poverty in its wake. But where there is a "strong state," policies can be implemented that greatly improve quality of life, provide effective family-planning programs, and motivate people to have fewer children.

## Notes

1. The theoretical formulations set forth in this study build on the pioneering work of A. F. K. Organski and his associates (Kugler et al. 1983; and Organski et al. 1984); but the findings in the present study draw a sharper connection between political capacity, policy implementation, and declining fertility.

2. The variables included were life expectancy, infant mortality rate, adult literacy, males in the nonagricultural labor force, primary and secondary school enrollments, urbanization, and GNP per capita.

3. A note on Indian statistical data is needed. Despite the massive size of the undertaking, widespread illiteracy, and the rural nature of the population, Indian demographic and social statistics have been found to be remarkably accurate. These data are obtained from (1) decennial census enumerations; (2) a National Sample Survey (NSS); (3) the Sample Registration System (SRS), a monthly enumeration of 54 hundred (1981) sample areas; (4) statistical gathering by individual government offices; and (5) sample surveys by private research organizations such as the Operations Research Group.

4. While data for the latter part of the 1970s are available for most socioeconomic variables, the only statewide information on literacy rate prior to the 1981 CBR measure comes from the 1971 census. Further, as Murdoch has demonstrated, variation in fertility is most closely related to social and economic conditions when the latter are measured about a decade earlier than fertility (1980, 37).

5. In global perspective, India falls near the low end when it comes to female autonomy, especially in the rural areas where the vast majority of the people reside. Women are more likely than men to suffer neglect as children and in most regions of the country have shorter life spans than males (Miller 1981). Traditional kinship patterns restrict women's social interaction beyond the home and limit their rights to inherit and own property. Arranged marriages are the norm and young wives generally go to live with their husband's family, falling under the domination of their mothers-in-law and other female members of the family. But as Dyson and Moore (1983) have demonstrated, a sharp dichotomy exists between north and south India in the degree of restrictions on a woman's personal activities. In no region of India

does the amount of female autonomy approach that found in the West, but in kinship patterns prevalent in the south women have greater freedom of choice and personal movement.

6. The measure of political capacity used in this article has been adopted from the index proposed by Organski and Kugler (1980, chap. 2). Their measure similarly controls for income but also adjusts for the fractions of the GNP derived from exports, agricultural production, and mineral production. These adjustments were not made in the present index of political capacity because they are more applicable to cross-national differences and do not apply within a single country. Moreover, India's economy is primarily agricultural, but the technological application in agriculture varies sharply between the states. As a result, adjusting for state domestic product originating in the agricultural sector would affect the ability to generate taxes.

In a more recent work (Organski et al. 1984, 63-79) the authors measure political capacity in terms of what they describe as "political cost," which is the difference between actual tax-raising performance of a government (adjusted as in the previous study) and what it might be expected to extract at maximum performance under conditions of total military combat. They hold that political cost is inversely related to capacity and thus can be used, albeit in a roundabout way, as an indicator of political performance. As with the earlier measure, this index of political capacity applies primarily to the international context and does not meet the requirements of the research question proposed here.

7. The beta coefficient indicates the amount of change—in standard deviation units—that would be predicted in the dependent variable, given a change of one standard deviation in the independent variable. The value of the coefficient indicates only the relative importance of the independent variables, not the exact proportion of variance in the dependent variable explained by each. When used in path analysis, these beta coefficients generally are called path coefficients.

8. Separate multiple regressions—with measures of urbanization and industrialization substituted for income per capita—not shown here demonstrated similar lack of relationship with CBR.

9. Among Indian states, this general finding is most strongly confirmed by fertility behavior in the state of Kerala. The state is among the lowest in India when it comes to measures of overall wealth but the leader in reducing birth rate. Kerala also has the most widespread education system, the highest literacy, and the most effective public health program, which has reduced the infant mortality to the lowest in the country. In the last two decades Kerala has gone through the most sweeping land-reform program in India, and in 1974 the Kerala government became the first state government in India to provide some form of pensions for agricultural workers. In Kerala the essential living conditions and access to family-planning services have improved as a result of positive actions by government in the form of redistributive policies that have effected the poor. These policies have increased opportunities for women and reduced the cost of fertility regulation, which in turn have raised motivation among the poor (the largest segment of the population in every Indian state) to reduce family size. In contrast, more affluent states such as Haryana and Gujarat, without social programs of this scale, have a significantly higher birth rate (Herring 1983; Ratcliffe 1977, 1978; Zachariah 1984).

10. Because the beta coefficients provide a rank ordering of the importance of independent variables, the explanatory value of one variable may be estimated relative to the others. In the equation

$$X_1 = -.65\, X_3 + -.34\, X_5,$$

the importance of $X_3$ will be $-.65^2 = .423$, in comparison to that of the $X_5$ value $-.34^2 = .116$ and, thus, three-and-a-half times larger.

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# POLITICAL CONTEXT AND ATTITUDE CHANGE

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*Political context has an important impact on individual attitude change. This is an analysis of the dynamic effects of contextual variables. Drawing on data taken from the American National Election Study (ANES) panel study, we demonstrate that the environment shapes the way the citizen views politics. While varying in degree, the results hold for a broad (county-level) and a narrow (residential neighborhood-level) definition of the relevant context. The patterns involved suggest that citizens' evaluations of candidates and parties are most directly influenced by what their neighbors are saying at the moment, that is to say, the content of current discussion. In contrast, citizens' self-identification evinces sensitivity to the more stable partisan character of the environment. The results indicate that the impact of social influence is crucially dependent on the nature of contemporary political debate and that the social setting serves as an intervening mechanism in the broader communication system and not merely as an exogenous source for political information.*

Citizens form opinions about politics by evaluating information that is filtered through the social environment in which they live. This filtering process biases the information to accord with standards held by friends and neighbors. Consequently, opinion formation is a social as well as a psychic phenomenon.

This intuitively appealing statement must be taken as a proposition. While much research supports this fundamental view, the overall pattern is far from compelling. Yet, the idea of social influence rubs the touchstone of politics; it centers on the relationship between the individual and the collectivity. At issue here is whether political attitudes are formed in an essentially atomistic way or whether they depend crucially on citizens' interactions with friends and neighbors. On one theoretical level, the question involves the root of public opinion. On another theoretical level, it gauges the individual as an elementary actor in politics. Is it sufficient to think of the citizen as an individual acting in a collective environment, or must one take into account the citizen's being defined by that environment as well? The matter deserves attention.

We assess the role that social context plays in shaping political views. Using multiple definitions of context and a panel survey taken during the 1980 presidential campaign, we are able to (1) identify social influence on attitude *change,* (2) compare its power with that of psychological factors, (3) explore subtleties in the influence process, and (4) extend our understanding of the link between the immediate social milieu and the broader political environment.

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## Background

The notion that one's social environment affects individual attitude formation and resultant political behavior has been tentatively accepted at least since Tingsten's early work (1937). While it is plausible that the associations between individual views and those dominant in the local political context might be generated by other phenomena, the scientific community's primary interest in contextual effects stems from their serving as evidence for interpersonal influence (Sprague 1982).[1] Empirical studies of contextual effects suggest that individual behavior and opinion formation are partly a function of the social environment. Berelson, Lazarsfeld, and McPhee's (1954) classic work on voting is different from most other studies because it utilizes longitudinal data. Their analysis reveals a bias in opinion change toward community norms. The most important subsequent evaluations of the basic hypothesis have relied on cross-sectional data. Warren Miller's (1956) seminal work, using data from the 1952 Survey Research Center national election study and county-level measures of political context, demonstrates that the political environment affects individual vote intentions independently of individual partisan identification. Further probing the same national election study, Putnam (1966) finds that social interaction, as structured by group membership, is a principal agent in the transmission of community norms. Brown's (1981, 1987) analysis of individual life histories yields additional evidence that individual political attitudes are affected by the political character of the environment. Weatherford's (1982) work on the neighborhood's influence on political attitudes reconfirms the contextual effect for microenvironments. In addition, comparative analyses further indicate a social influence process. The important works by Butler and Stokes (1969, 144–50) and Miller (1977) utilizing British data and by Langton and Rapoport (1975) studying urban workers in Chile are primary examples. Yet, while the lay of the land supports the social influence hypothesis, the typically modest observable effects do not always exceed standard statistical noise (for an explicit test—and a caution—see Bodman 1983). Further, because so much of the evidence comes from cross-sectional rather than longitudinal observation, a number of alternative hypotheses cannot be eliminated (Prysby 1976). In this article we shall present evidence from a dynamic perspective that fills out the picture.

The mechanisms through which social influence is felt have been variously portrayed. In the classic two-step model (Katz and Lazarsfeld 1955), opinion leaders carry messages to the less-involved. However, Robinson's (1976) important empirical analysis of political influence suggests that such one-way influence is rare and that instead the typical form of social interaction is the discussion group, each member of which carries ideas to others. Yet little is known about how such mutual social influence operates. First, it may be that individuals adjust their fundamental views in accord with the established political norms in their local environment and that any subsequent biases in opinion formation merely reflect this long-term conceptual frame of reference. Second, interpersonal influence may depend not on long-term conditions but instead on what we shall call *hot* communication—that is, the content of currently ongoing conversations, whether they reflect stable political biases or not. This hot communication may operate in two ways. On the one hand, it may reflect the tendency of members in the social environment to offer views consistent with their own beliefs and thus may shape, more or less continuously, the information that any member of the group encounters (for a careful and rich exposition, see Huckfeldt 1983a). On the other hand, McPhee (McPhee 1963; McPhee and Smith 1962) suggests a more subtle and indirect process based on social-reality testing, in which an individual takes information from external sources (most notably the mass media), forms a personal understanding, and then places it before peers for confirmation. When the understanding passes muster, the individual is on the way to a firmer attitude; when it fails, the individual seeks further outside information and repeats the test. Other things being equal, the net result is pressure to conform with collectively held views. In this model, the social context acts not as a source of political information but instead as an intervening agent.

Most of the evidence that supports the idea of social influence is essentially cross-sectional. Yet, if individuals are subject to others' influence, then the process ought to leave a trace in cross-temporal data. Further, the possible mechanisms of social influence will leave characteristic time traces, and longitudinal data will evaluate the different models. We shall examine a set of data on opinion *change* in order (1) to assess the empirical evidence for social influence and (2) to refine further our understanding of the mechanisms involved in political persuasion.

## The Data and Analytic Strategy

We investigate contextual influence on political attitudes by studying the dynamic impact of the social environment, defined in two different ways, on change in citizen attitudes both toward major presidential candidates and toward political parties. We analyze data taken from the 1980 ANES panel study in which individual-level measures of political orientations, feelings, and loyalties were elicited from a national sample in three waves: January, June, and September of 1980.[2] Thus the intervals between measurements include, first, the entire primary campaign—fiercely contested in both parties—and, second, the fall mobilization spurred by the national conventions and the early stages of the campaign.

We employ two definitions of context. The first, the macroenvironment, is measured by the long-term political orientation of the respondent's country (here the average proportion Republican or Democratic vote 1972–80). The second, the microenvironment, taps the local neighborhood. We rely on the respondent's self-report of perceptions of specific neighbors' partisan loyalties and vote intentions. These two definitions are conceptually distinct, the first encompassing a broader range of "local" phenomena while the second focuses on a specific set of conversational partners. In addition, by our using the pair we shall be able to avoid some dangers inherent in each one separately. The county-level data clearly do not represent anyone's local discussion context with any precision, and the bluntness of the measure does not permit subtle analyses. The self-report of neighbors' views is subject to respondents' manufacturing data to accord with their own views and does not represent relevant social contexts at the work place or elsewhere outside the immediate residential neighborhood. A consideration of these two measures of context and an analysis of their impact on attitude change will go a long way toward evaluating the magnitude and mechanisms of social influence.

Change is our tool to examine causality. If the social environment affects attitude formation, then the process should leave an empirical trace in sequenced observations on individual views. Let us say that we wish to model the change in an individual's evaluation of Ronald Reagan. Here we use a response to a thermometer rating as a measure of the respondent's opinion at two times (say January and June, or *t*1 and *t*2). The change in opinion, then, may be calculated as the difference between feelings toward Reagan as observed in January and June.

The straightforward analysis tests the proposition that such observable change may be a function of each respondent's social environment. The expectation is that individuals will align their views with those dominant in their political milieu. However, we wish to examine this empirical link while also taking into account a much more individualistic model of attitude change. Our model includes three substantive components. First, we consider the national movement toward or against the particular candidate. Such a factor is represented in the constant term of an estimation equation (and further refined by including a measure of the respondent's attentiveness to the campaign). In addition, ordinary information-processing theory suggests that individuals will be receptive to political information consistent with their own fundamental political attitudes and interests. For example, a conservative Republican will be expected to alter his or her evaluation more positively toward Reagan than will a liberal Democrat. For this reason, we include in our estimation models such factors as the individual's partisanship, ideology, race, income, education, sex, and age to represent relatively robust attitudes and political interests. Finally, attitude change should reflect previous attitudes toward Reagan. On the one hand, supporters should be biased toward positive information and opponents toward negative information; call this reinforcement. On the other hand, ordinary reequilibration mechanisms (often seen as regression toward the mean) should have the opposite effect. While these last two biases cannot be separated out in our data, their net effect must be considered and the respondent's previous attitude will be included in the model of attitude change. The estimation equations will take the form

$$\begin{array}{l}
\text{Change Reagan (1-2)} = \\
\begin{array}{ll}
\begin{array}{l}\quad\text{Constant}\\ + a\ \text{(attentiveness)}\end{array} & \left\{\begin{array}{l}\text{national}\\ \text{change}\end{array}\right. \\
+ s\ \text{(environment)} & \{\ \text{social influence} \\
\begin{array}{l}+ p\ \text{(partisanship)}\\ + i\ \text{(ideology)} + \ldots\end{array} & \left\{\begin{array}{l}\text{psychological}\\ \text{processes}\end{array}\right. \\
- r\ \text{Reagan(1)}, & \left\{\begin{array}{l}\text{reequillibration/}\\ \text{reinforcement}\end{array}\right.
\end{array}
\end{array}$$

where the thermometer ratings of Reagan are given time indexes; the social environment is measured by the political complexion of the individual's county or neighborhood; the psychological component is represented by partisanship, ideology, and so on; and the reequilibration/reinforcement mechanism is marked by the *previous* attitude toward Reagan.[3] Of particular interest will be the visibility of the contextual impact against a background of statistical noise and the relative importance of the environmental as compared to the psychological factors. In the first appendix we show that this disarming estimation equation represents a modestly sophisticated dynamic model of attitude change.

## The Macroenvironment: The County

We consider first the political complexion of the individual's county of residence. Of course, for many locations the county is a very rough approximation of the political environment—it may include a variety of subcultures whose effects are cancelled in an overall measure, and the composite measure may mask the genuine effects of social influence. Nevertheless, the county does present an indirect measure of social context in the sense that the probability of encountering different partisan arguments depends on the nature of the general environment (classically, the "breakage" effect of Berelson et al. 1954; for some empirical evidence, see Huckfeldt 1983b). Further, county-level measures of political character are readily available and represent the baseline of contextual research for much of the discipline. Our using this macrodefinition allows us to examine the oft-reported cross-sectional result from our longitudinal perspective, and it also serves as a bridge to individually specific definitions of the neighborhood.

We address two questions: (1) What are the magnitudes of the contextual effects occurring over time? (2) During which period of the campaign year are the contextual effects more pronounced? We begin to address the former by examining a fully specified model containing a straightforward contextual measure.

## The Magnitude of Contextual Effects

In Table 1 we present the estimates for a model in which the dependent variable is the change in the feeling thermometer for Reagan between January and September. There are 10 independent variables included in the model in order to represent the effects of both social and individualistic information-processing mechanisms. Note that there are no significance problems with the coefficient for the contextual term (Republican context), the proportion of the local Republican presidential vote as averaged since 1972. There are six variables whose coefficients are statistically discernable from zero in this

**Table 1. Change in Feelings for Reagan, January to September Full Model Specification: County Context**

| Variable | Parameter Estimate | Probability > T | Standardized Estimate |
|---|---|---|---|
| Straight Impact of Context | | | |
| Intercept | 15.11 | .13 | |
| January feeling | - .57 | .00 | -.58 |
| Republican context | 20.58 | .03 | .07 |
| Sex | -2.90 | .09 | -.06 |
| Race | -3.77 | .25 | -.04 |
| Education | - .61 | .09 | -.06 |
| Income | .19 | .24 | .04 |
| Partisanship | 2.87 | .00 | .24 |
| Age | .10 | .07 | .07 |
| Ideology | 1.22 | .13 | .06 |
| Information | .07 | .59 | .02 |
| $R^2 = .30$; Adj. $R^2 = .29$ | | | |
| Impact of Context in Interaction with Previous Feeling | | | |
| Intercept | 26.17 | .00 | |
| January feeling | - .73 | .00 | -.75 |
| Republican context in interaction | .31 | .05 | .19 |
| Sex | -2.84 | .09 | -.06 |
| Race | -4.16 | .20 | -.05 |
| Education | - .60 | .10 | -.06 |
| Income | .19 | .26 | .04 |
| Partisanship | 2.86 | .00 | .24 |
| Age | .10 | .07 | .07 |
| Ideology | 1.22 | .13 | .06 |
| Information | .07 | .60 | .02 |
| $R^2 = .30$; Adj. $R^2 = .29$ | | | |

model. Of those six, the contextual measure has the third largest standardized coefficient. Thus, the contextual measure has a greater impact on the *change* in feelings toward Reagan than all other variables except the initial feeling term and the respondent's partisan identification. Put differently, the impact of an individual's milieu on attitude change is greater than the effect of a respondent's sex, age, or education in addition to all those variables whose coefficients did not survive the significance test at all, such as the respondent's race, information level, income, and ideology.

When the contextual measure is specified in interaction with the respondent's lagged (January) feelings, the picture is even more dramatic. In Table 1 we present the estimate for such a model. Theoretical representations of social influence clearly take into account the relative distance between the individual's views and those of the group (see Huckfeldt 1983a and Sprague 1982). While this interactive estimation equation suffers from collinearity between the "interactive contextual" term and the "previous feelings" and thus produces imprecise coefficients, it most nearly reflects a full theoretical expression.[4]

The results presented in the first and second parts of Table 1 are in large part comparable except for the magnitude of the standardized coefficient for the contextual variable. Here, the standardized contextual coefficient approaches the magnitude of the standardized coefficient for partisanship. Although these basic results do not yet address the mechanisms of social influence, they do indicate that the effect of political context on attitude change is both statistically evident and substantively important.

Contextual influence is not limited to Reagan attitudes. We conducted similar analyses of attitude change for the time periods January to June (1-2), June to September (2-3), and January to September (1-3)[5] for evaluations of Reagan, Carter, the Republican party, and the Democratic party. With the exception of feelings toward Carter, the contextual effects are pervasive. In all but one of the regressions, the magnitude of the standardized coefficients for the straight contextual variable ranks at least fourth among all variables. Moreover, using the interactive specification of context instead, the contextual effect never ranks lower than third, surpassed in importance only by the effect of the respondent's partisanship and the previous attitude itself. The dynamics of context are clearly manifest.

### The Timing of Context

It is of interest here to ask when the effect of context is greatly felt. Is it during the initial period of the electoral calendar when ideas about the candidates and the parties are just beginning to circulate and judgements of and reactions to candidates are tentative and exploratory? Or is the real impact of context felt during the final stages of the campaign when the heat of the two-way electoral battle is on? The argument for the latter period is that context will have a greater impact when campaign activity is most ferocious. The argument in favor of the earlier period is that environmental influence is greatest when opinions are tentative and that, by the "endgame" of the campaign, the contextual effect will have already been felt. It is also possible that context will bear its imprint during the entire process of the campaign. In this case, the effect of context would be more appropriately read as a continuous process between January and election day.

In Table 2 we present the estimated coefficients for contextual variables, written as a straightforward variable and as an interactive specification (context times the value of the initial feeling thermometer variable). The parameter estimates

**Table 2. County Context and Change in Evaluations**

| Candidates and Parties | Time Interval<br>1–2 | <br>2–3 | <br>1–3 |
|---|---|---|---|
| Reagan | | | |
| Straight | 19.41*<br>(8.28) | 15.63<br>(8.44) | 20.58*<br>(9.49) |
| Interactive | .27*<br>( .14) | .25*<br>( .13) | .32*<br>( .16) |
| Carter | | | |
| Straight | -11.10<br>(8.84) | - 6.62<br>(7.91) | -11.56<br>(9.50) |
| Interactive | - .16<br>( .13) | - .12<br>( .13) | - .18<br>( .14) |
| Republican party | | | |
| Straight | 5.52<br>(7.16) | 15.79*<br>(6.90) | 10.13<br>(7.43) |
| Interactive | .10<br>( .11) | .21*<br>( .11) | .19<br>( .12) |
| Democratic party | | | |
| Straight | -12.86<br>(7.69) | - 8.88<br>(6.83) | -12.84<br>(7.38) |
| Interactive | - .20<br>( .11) | - .16<br>( .11) | - .24*<br>( .11) |

Note: All estimates are taken from fully specified models of the form shown in Table 1. Only the context coefficients are presented here. Standard errors of the estimators are shown in parentheses.

*Significant at the .05 level.

and their standard errors are included for all possible time periods.

Begin examining Table 2 by looking at the row on feelings for Reagan. To compare time periods, examine the coefficients for periods 1–2 (January to June) and 2–3 (June to September). Note that for both the straight and the interactive versions of the contextual measure, the greater impact of context was felt in the earlier period, that is, during the primary season. A similar tale can be told using change in feelings for Carter and the Democratic party as the dependent variables. The earlier period contains the largest contextual impact. However, the story is complicated somewhat by the results obtained using feelings for the Republican party. Here, the only significant impact of political context is found in the later period.

What does all this mean? Context is complex. Contextual effects vary across individuals as well as parties. Moreover, the effect of context varies with regard to the nature of the political attitude and the character of contemporary political debate. Democrats held the White House and the Congress for all of 1980. It is likely that the political discussion process was more clearly focused on the Democratic rather than the Republican party during the primary season, especially because media attention reflected the Democratic control of the Senate, House, and presidency as well as the ongoing Kennedy-

Carter battle for the heart of the Democratic party itself. However, after the primaries were over, the Republicans and the Democrats were clearly viewed as head-on combatants. When the Republican choice was clear, feelings for the Republican party, as conditioned by each respondent's political environment, began to congeal. Thus, context has its greatest impact when the focus of the campaign is clearest. For the incumbent party and for individual candidates for whom media attention can be focused intensely, the contextual imprint can be found early in the electoral season. However, for the out-of-power party, feelings remain less structured until the electoral season matures and the party's image becomes more distinct.

The complexity of contextual influence is underlined by a puzzle lying in the coefficient pattern of Table 2. Note that the effect of a Democratic county context on feelings toward Carter and the Democratic party shows a negative sign (that is, the more Democratic the context, the greater the negative change in Democratic evaluations). To be sure, these terms are not statistically discernible from zero, but it is unlikely that such a clear pattern was produced by chance. Such a counterintuitive pattern indicates complexity and suggests care in the use of macroenvironmental contextual data. In the second part of the appendix we provide an interpretation of this curiosity.

## The Microenvironment: In the Neighborhood

We now examine the effect that an individual's neighbors have on attitude change. By examining the influence of specifically named individuals on the citizen's political views, we shall be able to pursue a more subtle analysis of the mechanisms involved here. First, and by far most important, we shall be able to test whether the macrocontextual effect (measured by county political complexion) may plausibly be attributed to concrete social interactions. In addition, we shall shed some light on the micromechanism involved by showing that social influence depends not on indirect cues about social norms but instead on the content of actual communication and that the asymmetry of the influence fits nicely with the social-reality test (McPhee and Smith 1962; McPhee 1963) model of social interaction. Finally, we shall illustrate the susceptibility of partisanship itself to social influence.

A wistful tradition sets a high place at the table for the neighborhood to honor it as a guardian of democratic stability. Yet, the image of every citizen's being knit into a neighborhood political fabric just does not describe the life of most contemporary citizens. When asked if there were anyone in the neighborhood with whom the respondent socialized, only a modest majority (66%) ventured a name. Clearly, the myth of the neighborly American exaggerates the extent to which late twentieth century citizens find their friendships near their homes. Of course, given occupational and residential mobility patterns as well as the effects of the automobile revolution, such a result might be expected. More particularly with respect to politics, only about one-third of the sample could both name a neighborhood friend and identify either that neighbor's partisan orientation or current (1980) vote intention. Thus, when we examine social influence in the neighborhood, we must realize that the effect is practically limited to a minority (though a substantial minority) of citizens.

Nevertheless, this subset is of interest. If we can identify a social effect here, we add presumptive evidence for further social effects at the work place or among family or friends outside the immediate neighborhood. Thus, we shall look for social communication phenomena where

**Table 3. Neighbors' Partisanship and Change in Evaluations**

| Change in Evaluation by Time Interval | Neighbors' Partisanship | | Own Partisanship |
|---|---|---|---|
| | Republican | Democratic | |
| Reagan | | | |
| 1–2 | 7.4* | 2.6 | 15.3* |
| 2–3 | 3.0 | 2.6 | 11.5* |
| 1–3 | 8.1* | 2.8 | 16.3* |
| Republican party | | | |
| 1–2 | -1.7 | 3.3 | 19.8* |
| 2–3 | 5.8* | 2.7 | 9.2* |
| 1–3 | 4.9* | 3.2 | 14.4* |
| Carter | | | |
| 1–2 | -3.1 | -2.1 | 9.5* |
| 2–3 | -1.8 | 2.3 | 10.6* |
| 1–3 | -2.6 | .7 | 14.9* |
| Democratic party | | | |
| 1–2 | -3.8 | 1.3 | 19.6* |
| 2–3 | .5 | 3.8* | 11.6* |
| 1–3 | -2.0 | 3.3* | 15.5* |
| Anderson | | | |
| 2–3 | 1.3 | 2.8 | -3.2 |

Note: All estimates are taken from fully specified models of the form shown in Table 1. Only selected coefficients are presented here.

*Coefficients significant at .05 level.

they lie in our data and may guess that they exist elsewhere as well.

## Identifying Social Influence

First consider partisanship. Our basic argument is that when citizens discuss politics, perhaps at the most casual level, the social environment shapes the substantive content. Our expectation is that citizens, taking cues from their conversational partners, will encounter and more readily accept information consistent with the political character of the environment.

In Table 3 we mobilize the elementary dynamic model to estimate the effect of each citizen's *friends'* partisanship in shaping the citizen's views of the presidential candidates and the parties. The coefficients presented include those for the neighbors' partisanship and the individual's own partisanship. The full estimation equations incorporate, as well, an endogenous lag that captures the reequilibration-reinforcement composite and such intrapsychic guides as the individual's ideology, sex, race, income, and education. Each full equation is similar to that shown in Table 1, although not all coefficients are shown. Each estimation model is represented across the rows. The way to look at this table, however, is down the columns. By analyzing microdata we are able to isolate the impact of talking with specific individuals and thus may examine the differential impact of Republican or Democratic conversational partners in coloring information about both Republican and Democratic candidates and parties. Each coefficient reveals the bias in the citizen's information environment relative to that of other citizens who know nothing of their neighbors' political views.[6] Note, for example, that talking with Republicans clearly affected how the citizen altered his or her views toward

### Table 4. Neighbors' Vote Intention and Change in Evaluations

| Change in Evaluation by Time Interval | Neighbors' Vote Intention | | | Own Partisanship |
|---|---|---|---|---|
| | Reagan | Carter | Anderson | |
| Reagan | | | | |
| 1–2 | 9.1* | -.3 | -20.6 | 14.8* |
| 2–3 | 8.2* | -1.2 | -10.8 | 10.1* |
| 1–3 | 13.4* | -2.8 | -23.1* | 15.0* |
| Republican party | | | | |
| 1–2 | 0.0 | -.1 | -4.6 | 18.8* |
| 2–3 | 5.8* | 1.3 | -6.3 | 8.8* |
| 1–3 | 5.8* | -.4 | -7.9 | 13.7* |
| Carter | | | | |
| 1–2 | -8.4* | 8.1* | -2.4 | 8.3* |
| 2–3 | -5.9* | 4.9 | -.4 | 10.1* |
| 1–3 | -9.4* | 11.1* | -.2 | 13.7* |
| Democratic party | | | | |
| 1–2 | -4.2 | 3.6 | .3 | 19.7* |
| 2–3 | -5.4* | 6.2* | -9.2 | 11.1* |
| 1–3 | -7.9* | 7.8* | -7.3 | 14.9* |
| Anderson | | | | |
| 2–3 | -2.4 | -3.3 | 21.7* | 3.2 |

Note: All estimates are taken from fully specified models of the form shown in Table 1. Only selected coefficients are presented here.

*Coefficients significant at .05 level.

Reagan while talking with Democrats had little effect.

The results present uncertain evidence for social communication bias. A Republican context clearly shapes views toward Reagan and the Republican party; a Democratic circle modestly helps attitudes about the Democratic party; and the discussion context does little to Carter feelings. More generally, the coefficients are fairly small and many are submerged in the background estimation noise. In all, the pattern lacks the crispness one might expect.

This model, however, may miss the underlying process. Friends' partisanship represents a fairly stable background condition of the social environment. It does not tap the dynamic information flow that characterizes active political discussion. Consider, instead, the friendship circle's vote intentions. For the majority of partners, vote intention is isomorphic with partisanship. However, by considering those cases where friends have deviated from their partisan guides, we shall be able to measure more precisely the political content of the information network. Republican friends revealing a Carter preference and, more importantly, Democratic friends opting for Reagan should speak with different voice than loyal partisans. Indeed, the data suggest that this is the case.

In Table 4 we present the product of a parallel analysis, substituting friends' vote intention for partisanship. The social bias is more potent, takes on the correct sign, and is statistically discernible throughout the set of equations. As a straightforward test of the social interaction hypothesis, these estimates provide firmer support than the occasionally murky estimates based on friends' partisanship.

This basic result is important. While the contextual effect of the macroenvironment (the county's partisan coloration) may be generated by historical tradition or by local media or party organizations that are not part of the social-communication hypothesis, this evidence clearly demonstrates that the context's influence can be attributed directly to social interaction. The general lay of the land for the microenvironment has the same topography as that for the macroenvironment. Despite its apparently commonplace nature, the significance of this result should not be underestimated. The correspondence between the two sets of results suggests that the macroenvironmental results presented above (and so dominant in the scientific literature on context) represent the workings of a genuine social phenomenon.

## On Micromechanisms

The microcontextual data permit a finer statement about the nature of the communication process. By looking simultaneously at both friends' partisanship and friends' vote intention, we can produce some empirical leverage that allows us to differentiate between two versions of the contextual hypothesis. The theoretical question is whether the contextual effect is generated by subtle environmental cues that operate in a steady state way or, instead, by the hot communication of current information. Imagine a citizen whose friends are partisan Democrats but, given the politics of the moment, are now leaning toward a Reagan vote. The stable context model suggests that the citizen will have developed a Democratic bias in information processing, a sort of socially based partisanship. The hot communication model posits that, regardless of the stable characteristics, it is the fact that the citizen finds his or her friends talking up Reagan that is important—the citizen will experience a Republican context. The empirical question is whether it is neighbors' partisanship or neighbors' vote intention that most dramatically generates a bias in the information environment.

In Table 5 we display two representations of the analysis. Observe the *added* explanatory power of neighbors' partisanship, vote intention, and the combination of the two when compared to a baseline model comprised of the intrapsychic components. The numbers represent comparisons of adjusted R-squares in order to take into account varying degrees of freedom. In this representation it is clear that most of the social effect is generated by the neighbors' vote intentions rather than by their partisan loyalties. Once the impact of vote intention is taken into account, partisanship adds practically nothing. Further note that the extra explanatory power of the social environment runs in the range of 2% to 3% over that produced by an individualistic model. This increase is both statistically identifiable and worthy of note.[7]

In the lower half of Table 5 we show the impact of neighbors' partisanship and of neighbors' vote intention (the numbers represent the contrasting effects of full and homogeneous social settings).[8] For example, the difference between having three Republican friends and three Democratic friends had an estimated informational bias of −0.7 points on January–September changes in a Reagan evaluation. The similar effect for having three Reagan supporters as opposed to three Carter supporters in the social circle is 15.9 points.

The overall picture is striking: neighbors' vote intention dominates. Once we know for whom the friends intend to vote, their partisanship is entirely irrelevant. When it comes to shaping information about the candidates or the parties, it is clear that we observe the effects of transitory biases, hot communication, rather than the effects of a socially anchored partisanship. This is, of course, consistent with the social communication rather than the social cues model of social influence. Further, it suggests that attempts to model contextual effects based on long-term characteristics of the environment produce only pale imitations of the real thing. The long-term political history of the county is at best a measure of local partisan patterns. However, these results indicate that it is not the fundamental character of one's social milieu that matters; it is what people think and say at the moment that carries the day.

In addition, the coefficients in Table 5 indicate the strength of social influence. The effects of neighbors' vote intentions are substantial. In fact they compare favorably with the information-shaping power of the individual's self-proclaimed

**Table 5. Joint Impact of Neighbors' Partisanship and Vote Intention on Candidate and Party Evaluations**

| Change in Evaluation by Time Interval | Neighbors' Partisanship | Neighbors' Vote Intention | Both Neighbors' Partisanship and Vote Intention | Own Partisanship |
|---|---|---|---|---|
| Additional Explanatory Power over Individualistic Model (Percent Adjusted R-square) | | | | |
| Reagan | | | | |
| 1–3 | .9 | 2.5 | 2.8 | |
| Republican party | | | | |
| 1–3 | .6 | .3 | .9 | |
| Carter | | | | |
| 1–3 | - .1 | 1.6 | 1.6 | |
| Democratic party | | | | |
| 1–3 | .3 | 2.4 | 1.9 | |
| Anderson | | | | |
| 2–3 | - .1 | 2.1 | 2.1 | |
| Impact Coefficients in fully Specified Model | | | | |
| Reagan | | | | |
| 1–3 | - .7 | 15.9[a]* | | 15.8* |
| Republican party | | | | |
| 1–3 | .9 | 5.4[a] | | 14.4* |
| Carter | | | | |
| 1–3 | −4.2 | 23.4[a]* | | 14.2* |
| Democratic party | | | | |
| 1–3 | - .8 | 16.7[a]* | | 14.7* |
| Anderson | | | | |
| 2–3 | .3 | 25.3[ab]* | | 3.2 |

Note: All estimates are taken from fully specified models of the form shown in Table 1. Only selected coefficients are presented here.

[a] The contrast is between a Reagan and a Carter environment (leaving Anderson supporters to the side).

[b] This is contrast for Reagan-Anderson friends. The comparable coefficient for Carter-Anderson is 26.4.*

*Coefficients significant at .05 level.

partisanship. This pattern clearly suggests the value of considering social, in addition to individualistic, models of political change.[9]

Finally, the neighbor vote intention data provide a hint about the character of hot communication. Were the social circle an independent source of political information, then we should expect to observe a continuous impact over time. Both the macro- and microcontext data indicate that the effect depends on the national political debate. Further, the effects should be symmetric: Reagan supporters ought to move others toward Republican attitudes and away from Democratic; Carter supporters ought to move others toward Democratic attitudes and away from Republican. The first expectation is confirmed; the second is not (see Table 4). Given the nature of the 1980 campaign, the McPhee social-reality-test model predicts this asymmetry. Citizens in Reagan contexts would have had good news about Reagan and bad news about Carter confirmed by their peers. Those in Carter circles would find good news about Carter confirmed. However, given the relative one-sidedness of the Reagan presentation, these same individuals would find little bad news about Reagan to be confirmed by their friends. This twist in the evidence further indicates that the social circle is not an active independent force in politics but instead shapes information that comes from outside. Thus it is proper to think of the social environment not as a source of influence but as an intervening mechanism in a larger communication system.

## Context and the Development of Individual Partisanship

So far our inquiry has relied on the notion that individual partisanship is a fixed psychic state. To be sure, the cross-temporal stability of party identification dwarfs that of party and candidate evaluations (and, by an even greater margin, that of issue orientations). One may feel safe that self-identification is a more fundamental political attitude than are feelings about politicians and political ideas. However, partisanship is not absolutely inert, and its evolution may be affected by the environment. Again, our expectation is that partisanship will converge toward the context's complexion, either toward friends' partisan orientations or toward the content of ongoing conversations.

In Table 6 we present analyses similar to those just above, this time for change in the individual's own partisan identification. For comparability, the familiar seven-point construct is here scaled in equal intervals from 0 to 100, *strong Republican* to *strong Democrat.* Discovering that the change can be modeled at all is a mild upset: movement in partisanship is not merely a function of measurement noise. The *added* explanatory power depicted at the top suggests that the social environment plays an important part in guiding the citizen's self-concept. The overall increase of 2.4% is all the greater when one understands that the individualistic model produces only 5.1% of the variance in partisanship's change.

Unlike sentiments toward the parties and candidates, however, it is friends' partisanship rather than their vote intention that is the major operational factor. While a trace of the hot communication effect can be seen, especially during the primary period, its contribution pales in comparison to that of friends' loyalties. The time breakdown indicates that this impact is felt in the June–September period and hardly at all during the spring. Importantly, partisanship's sensitivity to the environment is not constant but depends crucially on the framing of contemporary political debate.

The power of social influence is also notable. The coefficients in the lower portion of the table suggest that the social

Table 6. Joint Impact of Neighbors' Partisanship and Vote Intention on Partisan Identification

| Change in Partisanship by Time Interval | Neighbors' Partisanship | Neighbors' Vote Intention | Neighbors' Partisanship and Vote Intention |
|---|---|---|---|
| Additional Explanatory Power over Individualistic Model (Percent Adjusted R-square) | | | |
| 1–2 | – .1 | .9 | .7 |
| 2–3 | 3.2 | .7 | 3.3 |
| 1–3 | 2.4 | 1.3 | 2.4 |
| Impact Coefficients in Fully Specified Model | | | |
| 1–2 | 1.1 | 13.0[a]* | |
| 2–3 | 13.0* | -1.1[a] | |
| 1–3 | 9.9* | 6.0[a] | |

Note: All estimates are taken from fully specified models of the form shown in Table 1. Only selected coefficients are presented here.

[a]The contrast is between a Reagan and a Carter environment (leaving Anderson supporters to the side).

*Coefficients significant at .05 level.

environment may move an individual about one-tenth of the scale toward his or her friends' attitudes. That is to say that a citizen imbedded in a homogeneous social circle will move almost one category of the party identification scale—say from weak Republican to strong Republican.

The data are consistent with the notion that self-identification relies on a different sort of social input than does candidate evaluation. Feelings toward the candidates are clearly more ephemeral in nature and describe an orientation toward others. Current, transitory, information plays the greatest part in influencing attitude change. A political self-definition, on the other hand, represents a more serious matter. Conversational content is not sufficient to generate change; instead, individuals respond to their friends' fundamental loyalties. This pattern suggests that self-identification depends on *imitative* rather than *cognitive* learning, with the citizen adopting friends' characters rather than reacting to what friends say. This understanding is buttressed by the timing of the social effect: it occurs during the June–September period, an interval when the contesting parties mobilize their loyalists. When political information is introduced to the discussion group in clearly partisan terms, the citizen's candidate evaluations are sensitive to the content of the political information, but the citizen's self-concept is sensitive to the carriers of the message.

## Conclusion

We are able to confirm the basic proposition that individuals' political views are subject to social influence. The expected effects appear in both the county- and neighborhood-level analysis and, crucially, stand out in individual attitude changes over time. Beyond ascertaining existence, the analysis shows that context operates through interactions with identifiable friends and neighbors: concrete personal relations rather than amorphous community norms are the proximate cause. Further, the empirical estimates indicate that social processes rival the power of psychological mechanisms in guiding attitude change. We are convinced that the social factor in shaping political information and political evaluations is real, tangible, and important.

We are also convinced that the process is complex. Both the macro and the micro data suggest that the contextual effect depends on the politics of the moment—the strength of the impact depends on the sorts of information that are being generated at different times during the campaign. The more precise micro data indicate that it is the content of contemporary conversations, rather than stable character of the social milieu, that generates the contextual phenomenon. They further show that social influence was most forceful for those in the discussion group who had short-term politics on their side (the Reagan as opposed to the Carter supporters). Thus context is not a constant force on politics, but one that shapes citizens' views of ebb and flow in the outside political world. Given our sparse theoretical understanding of the social influence process, none of this is altogether shocking. However, it is clear that context is an information channel, an intervening mechanism, rather than an independent agent in politics.

Finally, our results indicate that the dynamics of context itself may be complex as well. Our finding of influence stemming from neighbors' vote intentions rather than their partisanship certainly suggests a *contagion* model of social influence. As such, it is potentially explosive—even in the short run. When Reagan gained in popularity during 1980, his proponents became his missionaries to the uncoverted who, once turned, became missionaries on their own. Of course, our data are not adequate to support such inferences in detail; they can only indicate that it is the transitory rather than the permanent, the political attitude rather than the self-identification, that is the crucial force. On the other hand, the development of partisanship suggests that long-term change is not so volatile. When individuals' self-identities react to the environment it is not to the immediate informational content but instead to the partisan loyalties of their peers. Thus the more stable political character of the environment dominates long-term change. This, of course, has explosive implications as well. If the informational content affected partisanship, then, given fluctuation in the parties' political fortunes, we might expect contexts to shift their political hue accordingly. However, the dominance of long-term loyalties on change in partisanship indicates that as one side in the social circle begins to dominate, it will grow explosively, feeding on its own numbers. Again, our data are limited in their application to this inference in their being based on one year's events. Even so, the major impact of partisanship on partisanship is generated only during the fall campaign—it does not appear to be constant in time. Thus we might expect the self-strengthening social process to be limited not by its dynamic structure but instead by its infrequent manifestation in the time domain.

The overall picture of social influence is one of interdependence. The effectiveness of context depends critically on the sorts of information generated by the external politics of the moment. Further, while our data only allow us to observe a citizen's political environment shaping the available information, we must presume that the citizen similarly acts as an input to the political environment as well. Finally, contagion dominates the theoretical structure and its form is often complex. Thus, the citizen's social circle plays a discernible and important role in the development of political evaluations, but its part is one of a cast of players rather than that of an independent actor.

# Appendix

## The Dynamic Model

The elementary estimation equation used here represents a simple model of opinion dynamics. Let us say that we wish to model the change in an individual's evaluation of Ronald Reagan. Here we use a response to a thermometer rating as a measure of the respondent's opinion at two times (say January and June, or $t1$ and $t2$). Each score may range from 0 to 100.

Now consider how to model a positive change in the individual's sentiment toward Reagan. Let us say the individual will raise the evaluation on encountering positive information about Reagan. Also note that the score can only reach a maximum of 100, so we take that into account as well. Write the positive change in the following functional form:

$$\Delta^{+}\text{ Reagan}(1\text{-}2) = G(\text{positive information} \cdot [100 - \text{Reagan}(1)],$$

where $\Delta +$ Reagan represents the upward change Reagan(2) − Reagan(1), $G$ is a function indicating the individual's sensitivity to information, and the last term corrects for the amount of possible upward movement. Similarly write the downward movement as

$$\Delta^{-}\text{ Reagan}(1\text{-}2) = -G(\text{negative information}) \cdot \text{Reagan}(1).$$

Thus, we see that the observable change in Reagan evaluations is a function of the relative informational input and of an endogenous lag term. By this simple formulation we take into account the boundary effects (often approximated by logistic transformations) as well as reequilibration forces (often modeled by exogenously prescribed "forgetting" functions). The final result is a very simple expression.

Now our task is to model how the information that reaches a citizen is biased by the citizen's social environment while taking into account alternative biasing mechanisms. Consider three major mechanisms that will shape information (see text): reinforcement (or *f*eedback), *p*sychic guides, and the *s*ocial environment. Letting $f$, $p$, and $s$ be linear functions for feedback, psychological processes, and social influence, we may express the positive information that reaches the individual as

$$I = \text{constant} + p(\text{partisanship, ideology, } \ldots) + s(\text{environment}) + f(\text{Reagan}[1]).$$

Putting the pieces together produces an equation of the form

$$\Delta\text{ Reagan}(1\text{-}2) = \text{constant} + p(\text{partisanship, ideology, } \ldots) + s(\text{environment}) - r\text{ Reagan}(1),$$

where now $r = g(1 - f)$ and the constant, $p$ and $s$ actually being $g$ constant, $gp$ and $gs$. We retain the negative sign for $r$ (reinforcement and reequilibration) because in stable systems (and in our data) reequilibration typically dominates feedback. This, of course, represents a fairly simple model for empirical evaluation. In principle and if $f$ were zero, we could actually estimate the proportion of positive information by dividing through by $r$. However, assuming $f$ to be zero is implausible and in any case the estimate for $r$ will be fraught with nonrandom measurement error. A more reasonable approach is to take the estimates for $p$ and $s$ as measures of the relative biasing of the information environment associated with the psychological and social mechanisms. These coefficients will prove central to this research.

## County Context

The macro and micro measures of context differ qualitatively. To grasp the differences, note the following anomaly. For the macro measure, Democratic environments (in their historical traditions) were associated with lowering evaluations of Carter and the Democratic party. Conversely, Democratic discussion environments (as marked by the vote intentions of particular neighbors) were associated with rising evaluations of Carter and the party. These results conflict and require explanation. Fortunately, the results yield additional insight into the nature of the measures themselves as well as their potential for further use.

Because county context is the most commonly employed measure of the political milieu, it is vital that we understand what its use implies. Is it a true proxy for the typical discussion environment, or does it represent some other aspect of political life? Our results indicate that the county-level measure does not simply mirror the discussion environment (otherwise the estimates using the micro data would correspond in sign). To appreciate the problem, recall that the county measure is constructed as the average Democratic (and, likewise, Republican) presidential vote for each county separately from 1972 to 1980: it marks the partisan tradition of the local environment. This can have only an indirect effect on the discussion environment of particular individuals (the bivariate correlations are modest). Norms in the environment will normally favor the Democratic party in areas where Democratic support has historically been heavy. However, those norms may not always be met by Democratic candidates: it is entirely possible that a traditionally Democratic environment will spur a short-term rejection of a presidential candidate whose posture fails to conform with traditional expectations.

In 1980 Jimmy Carter hardly represented the Democrats' Democrat. His governing philosophy and political failures seemed ill-tuned to the Democrat's song. Recall that 1980 was a problem-ridden year for the Democrats and their leadership. To many citizens, foreign and domestic problems seemed particularly frustrating. American hostages were being held in Iran, and the economy suffered from both inflation and unemployment. It is plausible that those living in traditional Democratic contexts would have had unusually high expectations for a Democratic president (ones not fully captured in our model's controls) and thus suffered from unusually great disillusionment. Close examination of the survey data indicates that respondents living in Democratic environments reduced their (relatively higher) evaluations of Carter and the Democratic party more than those respondents living in Republican environments. Further, the political year was shaped by an unusual insurgency against the incumbent president: Kennedy claimed that Carter did not represent the true historical traditions of the Democracy. Notably, his campaign against Carter achieved its greatest success in the party's traditional strongholds. Correspondingly, the puzzling macroenvironmental impact was strongest during the bitterly divisive primary season. From this post hoc perspective it is understandable that the macrocontextual data seem to operate in perverse ways.

This interpretation is supported by an analysis of attitude change toward Kennedy. It reveals that the effects of the discussion environment, for both county and neighbor definitions, are positive—as expected—for change in evaluations of Kennedy throughout the year. The macrocontextual influence is similar to that for Reagan evaluations during the primary period, though it understandably diminishes after Kennedy's campaign expired. One must suppose that our results for the Carter candidacy are unusual: under normal conditions a Republican's Republican and a Democrat's Democrat benefit from a contextual bias in the commonly expected manner.

What can be concluded regarding the use of the county-level measure of an individual's context? The historical-aggregate measure does reflect the probabilistic inclinations of the discussion environments, and its use will normally make sense. However, under unusual circumstances, when voter expectations (that are historically dependent) are not satisfied by party and presidential performance, the county-level measure represents something other than a simple partisan bias in the discussion environment. In this latter case, it can reflect the national and local traditions of politics from which contemporary parties, politicians, and events are judged.

## Notes

This paper is the result of a genuinely joint venture. Both authors share equally in responsibility for the results and the interpretations.

1. Such institutional factors as differential party organizational strength and local media bias suggest themselves as alternatives. Empirical analyses of their impact over a quarter of a century indicate that the social influence considered here is not spurious. A fuller description of this work is under preparation.

2. We are grateful to the Inter-University Consortium for Social and Political Research for providing these data. Neither that organization or the ANES's principal investigators bear any responsibility for our errors of interpretation.

3. The inclusion of the full specification gives an edge to the intrapsychic mechanisms. It is arguable that many individual attitudes can be attributed to the influence of the social environment. Our purpose is to evaluate the power of the social component while bending over backwards to allow the individualistic model full play. In a sense we do this because the individualistic model dominates current work in the field and we need to present the strongest case for the addition of a social component to complement the theoretical structures.

Note that it is the *change* in attitude that is on the left-hand side of the estimation equation. A similar equation might be produced that put only the attitude (at $t2$) on the left, with the effect of extracting some consistency of the previous attitude (at $t1$) placed on the right-hand side. The substantive coefficient estimates would remain exactly the same, and the variance accounted for would jump about 20 points (in Table 1 the R-square is about.52). Such an alternate specification misleads, however, because the extra boost in predictive power is generated entirely by attitude consistency and not by the substantively interesting variables. We are not trying to account for attitudes (at $t2$) but rather to examine observable change in attitudes. The "change" format is used here because it is explicitly produced by the elementary model derived in the Appendix and because it emphasizes the dynamic character of the evidence being considered.

4. The dependence of the contextual effect on the individual's previous evaluation is a powerful theoretical tool. Unfortunately, it is statistically impossible to separate the "straight" from the "interactive" effects because the collinearity of their estimators produces great imprecision. A number of tricks, including ridge regressions and a reparameterizing of the magnitude-dynamic components proved of no avail. Here we present the two specifications separately to give a hint of the potential power of the interactive specification.

5. Examining the two intervals separately and then the overall change provides different sorts of advantages. Of course, looking at the spring and fall intervals permits an identification of specific political events with the observable influence. On the other hand, an analysis of the full sweep of change provides evidence of the effects of the entire campaign on attitudes that were, more or less, at equilibrium in January. The difficulty with the June measurement is that it might reflect relatively transient attitudes affected by current events and as such provide an unstable baseline from which to assess change.

6. Each respondent was asked to name up to three neighbors with whom he or she socialized (in any way). After supplying some further identifying information, the respondent was then asked to give each specifically named neighbor's partisanship and current vote intention. The measure is, of course, fraught with possible inaccuracies. In particular, we might expect any respondent to misperceive the environment as being more compatible than it really is. Ordinary selective perception mechanisms may operate here. Further, the neighbor will have a "sociability" incentive to seek common ground with the respondent and thus appear more agreeable than would truly be the case. However, because we are interested in subsequent behavior, our tapping the individual's perceptions may be a particularly suitable tactic because it abstracts away such side issues. Further work on how these perceptions are formed will be of great interest: early returns from Huckfeldt and Sprague (1986) are promising.

Also elicited from the respondent was an indication of how often the respondent discussed politics with each neighbor. Conceptually, one would expect that the introduction of such descriptive information would tighten our analytic hold on neighborhood social influence. It does not. It may be that social influence is transmitted by means other than social contact. However, this and other previous work with these and similar measures bring into doubt the utility of the empirical measure (often social influence is greater when no political discussion is reported; sometimes influence directly linked to specific political debate has a discernible contradictory impact; sometimes not). It is not at all clear that respondents agree about what constitutes a *political* conversation. It may well be the case that much political influence gets casually imbedded in everyday chats while washing the car or taking out the garbage.

The numerical coefficients reflect the full potential effect of being in an environment of three Democratic or Republican friends (the effect for each friend is about one-third of the coefficient in the table) in the fully specified model. Thus, for Table 3, the estimated effect of having three Republican friends on the respondent's evaluation of Reagan is 7.4 points while that of having three Democratic friends is (a noisy) 2.6 points. Of course many respondents could identify the politics of only one or two neighborhood friends. Fortunately for interpretation, the relationship between social influence and the number of Democratic or Republican friends is approximately linear. That is to say, for example, that the effect of having two Republican friends is about twice the effect of having only one (with the third adding slightly less than his or her full share). The variables are scored (0, .33, .66, 1.0) when the respondent volunteers none, one, two, or three neighbors as Democrats or Republicans respectively. Partisanship is similarly scaled 0–1: strong Republican to strong Democratic or the reverse, appropriately. Thus, the social variables are here scaled so that their *range* is equal to that of partisanship and direct comparison is made easy.

7. While the "socially" generated *added* explanatory power may seem relatively small, it must be noted that this format assumes that all "individualistic" factors such as partisanship, ideology, and the previous attitude arise independently. Their theoretical exogeneity is not at all clear (see the empirical results for partisanship). It is likely that much of the "individualistic" component merely reflects the previous workings of social phenomena that have long since been stored in the individual's psyche.

8. Here the actual numbers reflect the *difference* in effect between having a circle comprised of three Democratic (or Carter) friends or having three Republican (or Reagan) friends. The Anderson comparison is similarly constructed.

Because the estimates were obtained jointly, they suffer from a good deal of collinearity, and their standard errors range over about 5 points. Thus, those coefficients of less than 10 points are not, strictly speaking, discernible. Care should be taken in overinterpreting the results.

9. Though the weight of the evidence lies heavily on the side of social influence, one can never be sure. The contextual effect persists when one simultaneously takes into account the individual's partisanship, political interests and attitudes, and previous feeling toward the specific candidate or party. Nevertheless, one can always argue that the evident social effect merely indicates a more complicated individualistic, psychological phenomenon: individuals choose neighborhoods and their friends or misperceive their political character in a way that reflects psychological leanings not measured by the standard variables. Possibly so; it will never be possible to eliminate such argument. Yet special attention must be given to exogenous rather than endogenous explanations. Instead of arguing that people like the candidates and parties because they like the candidates and parties, the social communication tack seeks foothold outside the individual's psyche.

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# RECKLESS RATIONALISM AND HEROIC REVERENCE IN HOMER'S *ODYSSEY*

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*A decision-theoretic analysis of the central incident of Homer's* Odyssey *reveals the insufficiency of rational calculation as a guide for political prudence. Surprisingly, the poet distinguishes between two rational and formally identical calculations in no uncertain terms; he condemns one as utter recklessness and praises the other as consummate wisdom. I maintain that this discrepancy is neither an artifact of sloppy editorial patchwork nor the result of a "homeric nod" but instead points toward a politically significant distinction as yet obscured by a merely rationalistic perspective. The recklessness of Odysseus' crewmen, who deliberately slaughter sacred cattle to forestall starvation, consists in their rationalistic transgression of the limits of reason. These limits are most evident in the defiance of commensurability that characterizes the sacred. The wisdom of Odysseus, by contrast, is manifest in his learning to temper reason with respect for the sacred. By virtue of reverence, Odysseus wins his struggle to preserve his* psychê, *home and regime.*

Homer's epic poems have attracted little attention among modern students of politics for many reasons, not the least of which is the argument that these works are adverse or, at best, irrelevant to contemporary liberal democracy. To be sure, Homer's polities are tribal, localized, and monarchical, while our own is pluralistic, continental, and democratic. Moreover, certain qualities of character celebrated in the poems, which consequently stimulate emulation in the reader, may be of dubious value as equipment for democratic citizenship. One thinks instantly of the magnificent thirst for glory that virtually epitomizes the Homeric hero but which in modern times has come to be regarded as nothing less than political dynamite. Tocqueville (1945, 2:63), for example, warns against the study of such "aristocratic" literature, as having "no bearing on [democratic] social and political needs," but tending rather to lead its enthusiasts to "perturb the state in the name of the Greeks and the Romans, instead of enriching it with their productive industry." Thus the great commentator on the American polity would question the vitality of any democratic regime bent on nursing a race of would-be Achilleis. In response to this legitimate and public-spirited concern, I intend to expound a central, though neglected, lesson of Homer's *Odyssey* that is both relevant and beneficial to liberal democracy. I hope to do so, moreover, in a manner that Tocqueville, one of the great critics of rationalism in politics, would himself find persuasive.

As I see it, the contribution of the *Odyssey* to liberal democracy consists principally in its critique of rationalism.

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By rationalism I mean the presumption of the ultimate hegemony of reason in the constitution of wisdom.[1] Hayek (1960, chap. 4), Oakeshott (1962, 1-36), and Gilder (1981, pt. 3) have made significant contributions toward identifying the precise character of the threat posed by rationalism to liberal democracy. But their common root lies in Tocqueville's (1945, 1:12) trenchant observation that "[political] liberty cannot be established without morality, nor morality without faith." Despite such warnings, contemporary rationalists remain undaunted. Much of the Law and Economics movement, for example, is directed toward establishing legal principles on grounds of efficiency, as if economic calculations of social wealth had more authority than "self-evident" inalienable rights (Posner 1977; Rubin 1977; cf. Aranson 1984). The philosophical underpinning of this rationalistic counteroffensive is most powerfully expressed by Rawls (1955). Rawls attempts simultaneously to vindicate morality and to establish the ultimate authority of reason by subsuming the particular instance (or "act") of moral propriety, in which utilitarian considerations are agreed not to be decisive, under a general practice ("rule") which, he claims, enjoys a utilitarian authorization. The pious acts of a believer whose faith is secured by Pascal's wager would, I think, perfectly exemplify Rawls's argument. If pious acts are tenable on this rationalistic basis, it would follow that the last link in Tocqueville's contention is false. But Hayek's (1976, 17-23) critique of rule utilitarianism may be understood as calling Pascal's bluff. Hayek notes that the consequences of observing a particular rule will vary as the constellation of other practices observed in society varies—even at the margin. But the utilitarian warrant for one such constellation of contextual practices over another is by no means evident. Owing to the sheer complexity of society, Hayek argues, utilitarians can never give a sufficient reason for a particular rule because any calculation of its consequences must *make assumptions* concerning the contextual practices that in part determine its utility. The same point applies to Pascal's wager: apart from a *prior faith* in a divine providence that has prepared Heaven as a place of reward for the righteous and Hell as a place of punishment for the unrighteous (and not the reverse), the wager—and moral propriety—is by no means compellingly rational. It appears that there is something to Tocqueville's contention after all.

This result will not surprise students of political philosophy. In lectures published posthumously, Leo Strauss (1979, 1981) cogently argues that "philosophy has never refuted revelation." He identifies the conflict between Greek philosophy and biblical revelation as "the secret of the vitality of Western civilization" and recommends that philosophers remain "open to the challenge of theology." If Strauss is correct, there is no warrant for assuming the comprehensive sovereignty of reason. One wonders, however, how the Greek philosophers, whom Strauss holds in the highest regard, might have lived up to his standard, lacking as they did access to the biblical revelation. In Homer's critique of rationalism, I believe, we shall find a nonbiblical influence that may help account for the recognition among subsequent Greek philosophers of the limits of reason.

In the *Odyssey* Homer intends[2] to elevate Odysseus to a rank at least equal to that of the illustrious Achilles, the hero of the *Iliad*. He presents the case for Odysseus' superiority by recounting the harrowing, though ultimately successful, return of Odysseus to home and throne after the Trojan War. Odysseus' adventures, the obstacles he confronts and overcomes in winning his return, summon the special excellences that define him as a hero. The most celebrated of these excellences is, of course, Odysseus' remarkable intelligence. But the most critical of these excellences, I shall argue, is reverence. By *reverence* I mean the correct perception of and respect for the sacred, which by its nature defies rational commensuration. As we shall see, it is by virtue of reverence that Odysseus resists certain temptations to rationalistic excess that would otherwise arrest his heroic return.

The commentators have not, in my view, paid sufficient attention to the important strain of antirationalism implicit in the poet's celebration of Odysseus' heroic reverence. On the contrary, they regard Odysseus' greatness as consisting exclusively in his intellectual resourcefulness. Even the most insightful resolve the age-old controversy—as to who, Achilles or Odysseus, is the "best of the Achaians"—in simple terms of brains versus brawn, shrewdness versus honor, or guile versus force (Clay 1983, 96–112; Nagy 1979, 35–58; Stanford 1985, chap. 5). It is a commonplace among homeric scholars that one finds crafty intelligence first elevated to heroic respectability in the *Odyssey*. It is true that Odysseus' characteristic reliance on wily tricks and clever deceits is scorned in the earlier *Iliad* (see, e.g., 9.312–13), but this scorn reflects the view of Odysseus' peers at Troy more than that of the poet. But if Odysseus' intelligence is not unappreciated in the *Iliad*, neither does it provide the basis for his heroic elevation in the *Odyssey*. The most pervasive evidence suggests that Odysseus is unable by cleverness alone to compete successfully for heroic honors with the less versatile but nobler Achilles. Odysseus' extraordinary intelligence is amply evident throughout the *Iliad*, and yet the poet devotes that work to the heroic preeminence of Achilles.[3] It would seem that insofar as Odysseus' distinctiveness depends exclusively on the scope of his intellect he must remain eclipsed by Achilles. To rival the *Iliad*, that is, to reveal the superiority of its own hero, the *Odyssey* must do more than simply catalogue the further exploits of the wily Odysseus. A fresh study of this work will, I believe, reveal that Homer does not rest Odysseus' heroic credentials upon a twice-told tale of brains versus brawn. In the *Odyssey*, he takes a different tack. Homer chronicles the development in Odysseus of reverence, which moderates and thus guides his remarkable intellectual prowess. Thus, in contrast to the view currently prevalent among homeric scholars, I hope to show that it is not chiefly the resourcefulness of Odysseus' intellect but rather his recognition of its limits that settles the Ithakan's claim to an epic poem of his own.

## Up from Rationalism: Odysseus as Epic Hero

To appreciate Odysseus' development into a hero of truly epic calibre, one must first of all reckon with the poet's opening allusion to Odysseus' sacrilegious plundering of the sacred temples of Troy (1.1–2). Evidently, the resourceful Odysseus proves to be very much the "typical hero" at Troy, employing the most brutal force for the sake of self-aggrandizement. Odysseus' outrage calls to mind Achilles' mutilation of Hector's corpse, which likewise manifested a heroic spiritedness unquenchable even in final victory (*Iliad* 24.14–22). As a hero of this conventional sort, however, Odysseus cannot surpass the glorious Achilles. Indeed, Odysseus' name is not even mentioned in the poet's prologue (cf. *Iliad* 1.1). Only in the *Odyssey*, which recounts Odysseus' experiences after his departure from Troy, does the Ithakan king truly come into his own. The poet prefaces this disclosure, however, by introducing the Odysseus that was—an Odysseus who was, if anything, the opposite of reverent.

As the prologue continues, we learn

that Odysseus did not persist in this irreverence. On the island of Thrinakia, in the course of his return to Ithaka, Odysseus stood alone against his mates' violation of the sacred cattle of the sun god, Helios. As a result the men perish, we are told, while Odysseus alone succeeds in returning to Ithaka. If, as the Thrinakian episode suggests, Odysseus' late-blossoming reverence is an important component of his heroic excellence, then Odysseus' full claim to rival Achilles will come to light only as the narration of the *Odyssey* unfolds. Only there, and nowhere else, will we learn how the irreverent swashbuckler from Ithaka came to be remarkable above all for his godly reverence.[4] It is, accordingly, in hope of better understanding this surprising development that the poet invokes his divine Muse (1.1–10):

> Sing through me, Muse, of that versatile man,
>   who so far
> strayed when he ravaged Troy's holy citadel.
> Though he saw the cities and came to know the
>   mind of many men,
> in his heart he suffered many pains at sea
> as he strove to win his soul and his mates'
>   return.
> But he could not save his mates, though he
>   longed to.
> On their own they perished on account of their
>   own recklessness,
> the fools. The cattle of exalted Helios
> they devoured, and He took away the day of
>   their return.
> Tell us of these things, from somewhere,
>   Goddess, daughter of Zeus.[5]

Odysseus' return to Ithaka is not merely a topographical achievement. As we gather from the prologue, Odysseus undergoes a radical change of heart in the course of his wanderings. This change, I suggest, is the focal issue of the *Odyssey.* It is significant that Homer's Muse responds to his prayer to begin "from somewhere" by guiding him in a reordering of episodes that breaks with strict chronology (cf. 23.310–38). Homer begins "in the midst of things," narrating some events as they unfold and others by means of flashback. As a consequence of this reordering, the poet achieves a literary "hat trick": the tale of the Thrinakian crime and punishment is positioned literally at the center of the poem, at the rhetorical climax of Odysseus' tale to the Phaiakians and at the threshold of his return to Ithaka. Thus the architecture of the *Odyssey* indicates a remarkable poetic investment in the Thrinakian adventure and, accordingly, in the lesson that we are meant to draw from it.

Nevertheless, the importance of Thrinakia is commonly depreciated by the commentators. Even the most scrupulous, whose watchword is "to interpret Homer out of Homer," find it difficult to take Helios' cattle seriously. Jenny Strauss Clay (1983), for example, holds against Homer and in favor of the "fundamental innocence" of Odysseus' shipmates (p. 230). She maintains that the poet's judgment that the crewmen lost their lives through their own responsibility is not "borne out by the account of the destruction of Odysseus' men later in the *Odyssey*" (p. 36). Perhaps it is supposed that by acts of recklessness (*atasthaliai*) Homer necessarily refers to conduct lacking deliberate and rational consideration. When one discovers that the crewmen's slaughter of the sacred cattle does not issue from thoughtless impulse but is startingly well reasoned, it is natural to question the validity of Homer's indictment. In my view, the best response to this tendency is to make the Thrinakian episode the centerpiece of a fresh examination of the *Odyssey.* On this basis I shall try to show how Homer's account in fact establishes the culpable recklessness of the crewmen and substantiates the dawn of reverence in Odysseus.

Doubt will remain, however, as to the heroic standing of Odysseus' reverence—which sets him in opposition to his crewmen—as long as it remains unclear that the crewmen's decision to slaughter sacred cattle, despite its rationality, is utterly reckless. Our task is further complicated, however, by the discovery that Odysseus later arrives at a similarly rational decision, inducing Athena—the goddess of wisdom—to certify and praise his good judgment (13.290–98). As we shall see, Odysseus and his shipmates adopt formally equivalent decisions, each choosing what game theorists call the *dominant strategy.* Nevertheless, Homer distinguishes between the two identically dominant strategies in no uncertain terms, condemning one as reckless and praising the other for its wisdom. As matters stand, therefore, we understand neither the grounds upon which the poet condemns the recklessness of the crewmen's rational choice nor those upon which he makes Athena commend Odysseus' good judgment. If we are to learn what, in Homer's estimation, makes the man Odysseus truly heroic, it is necessary to probe deeper into the details of each of the decisions to determine on what basis they might properly be distinguished.

## Homer's Ambivalence toward Rational Choice

Odysseus' shipmates learn of the prohibition against slaughtering Helios' cattle from Odysseus, who heard it first from the prophet Teiresias and then from the sorceress Kirke, the daughter of Helios (11.110–15; 12.137–41). It was by direction of Kirke that Odysseus set out for the underworld to consult with the soul of the departed Teiresias concerning the necessary provisions for his return to Ithaka (10.490–95). At the brink of Hades, he encounters the famous seer. Teiresias tells Odysseus that if he hopes to win his return he must above all be willing "to check [his] heart" (*thymon erykakeein,* 11.105). Teiresias specifically cautions Odysseus about Thrinakia. No matter what, Odysseus and his men must leave unmolested the herds of Helios, which roam there. For any who violate the sanctity of Helios' cattle, Teiresias prophesies doom. Teiresias' warning admonishes Odysseus unmistakably to control his appetite. But this interpretation does not exhaust the significance of the prophecy. The full range of meaning of the word *thymos* requires a more comprehensive interpretation. Thus, Odysseus must not merely control his desire for food but, more importantly, his *furor,* or love of fame. What Teiresias demands is literally an all-encompassing "change of heart." Odysseus' conduct on the island of Thrinakia is the acid test of a more encompassing change of heart that Teiresias prophecies as the key to Odysseus' final return.

It is after an especially harrowing day's sailing that Odysseus and his men finally disembark at Thrinakia. Although the Ithakans escape the Sirens' treachery unscathed,[6] they are not nearly so fortunate in their subsequent navigation of the narrow strait between Skylla and Kharybdis. In order to skirt the lethal vortex of Kharybdis, the relentless whirlpool, the Ithakans must sail close under an opposing cliff, the dwelling of the unspeakably monstrous Skylla. Skylla snatches six men from the deck of the passing ship. As the ship races on, the crewmen witness what Odysseus calls the most grievous spectacle of their journey: the writhing and screaming of their helpless comrades as Skylla devours them alive (12.255–59). At last achieving a safe distance, Odysseus' haggard crew compel him to make for port (12.297). The nearest island is Thrinakia. As he submits, Odysseus informs his men of the warnings of Teiresias and bids them to swear an oath under no circumstances to violate Helios' sacred cattle. The men vow to leave the herds unmolested and go ashore.

That very night a tremendous storm sweeps across Thrinakia. Inauspicious winds trap the Ithakans on the island for a month. It takes considerably less time than that, however, for the men to

**Table 1. The Rational Choice of Odysseus' Comrades**

| | Circumstances | |
|---|---|---|
| Strategies | The gods fail to cooperate: Crime goes unpunished | The gods cooperate: Crime is punished |
| Violate the sacred cows | Feast scot-free[a] | Die (quickly) at sea |
| Respect the sacred cows | Slow death by starvation | Slow death by starvation |

Note. Significantly, there is no text to support the composition of a similar decision matrix for *Odysseus*.

[a]Eurylochos assuages any guilt feelings the men might otherwise associate with this outcome by promising to erect and equip a temple to Helios when they return to Ithaka (12.345–47).

exhaust their meager stores. They turn in desperation to fishing and hunting fowl, but without success. Hunger wears them down. Acknowledging finally his own lack of resource—a critical step for "the man of many ways" (*andra . . . polytropon*, 1.1)—Odysseus leaves his men to seek guidance from the gods (12.333–34; cf. 12.38). In his absence, the crewmen "Hatch a wicked plot" to satisfy their hunger by slaughtering Helios' cattle. Eurylochos, Odysseus' second in command, addresses the men in terms they find irresistibly persuasive (12.339–52):

> "Heed my words, mates, for you have been
> ill-used!
> All deaths are hateful to wretched mortals,
> but to die of hunger is the most piteous way
> to meet one's fate.
> Let us, then, carry off the best of Helios'
> cattle. . .
> But if, in his rage for his straight-horned
> cattle,
> He would have our ship destroyed, and other
> gods back him up,
> then I heartily prefer to die at once inhaling a
> wave
> than to waste to death on a desert island."
> So said Eurylochos, and the other mates agreed.

Eurylochos' speech invites his mates to compare the respective consequences of reverencing and of violating Helios' cattle. Whatever the men decide, Eurylochos notes that there are two, and only two, possibilities. Either the other gods make common cause and honor Helios' claim to punish anyone who harms his cattle or they do not. What makes Eurylochos' argument so persuasive is his revealing that, in *either* of these possible circumstances, breaking the oath and slaughtering the cattle is the superior choice. This is perhaps most easily seen with reference to the decision matrix in Table 1, which is constructed directly out of the alternatives and consequences as they are formulated in the text.

It is Eurylochos' contention that the men will be better off acting as he recommends, regardless whether the other gods honor Helios' claim against them, as we see from the table. If the gods should fail to cooperate in punishing the men—such divisiveness among the gods is not unprecedented, as any veteran of the Trojan campaign knows—then slaughtering the cattle is clearly preferable to the alternative. In this case, the men will satisfy their hunger and pay no penalty whatever. But if, on the other hand, the gods should cooperate in their punishment, then slaughtering the cattle is *still* preferable to the alternative; for then their violation can be expected to result in a quick death, after a fine meal, instead of a slow death "by inches." No matter which circumstance actually comes to pass, then, the crewmen are better off slaughtering the sacred cattle. Whenever any strategy enjoys this superiority over all its alternatives in every possible circumstance, game theorists describe it as "dominant" (Owen 1982, 22–23). Of course, dominant strategies are not always available, but when one is available it would appear almost unthinkable to choose anything else.[7]

In the case of the crewmen's choice, however, the poet of the *Odyssey* would seem to disagree. Homer condemns their rational choice as utterly reckless (1.7, 12.300). By what right can one assert the "utter recklessness" of a rationally dominant strategy? Or are we simply to dismiss the poet as a rank misologist? The answer to this question is emphatically no. Homer's sympathetic treatment of clear reasoning is manifest throughout the work. But perhaps nowhere is it so evident as when Athena praises Odysseus as "by far the best of all mortals in counsel" for his resolve in concealing his identity (13.297–98). The significance of Odysseus' ironic dissimulation, particularly in its departure from the customary behavior of the "typical hero," should be carefully noted. As against his earlier preoccupation with glory and self-aggrandizement, Odysseus now contrives all manner of means, even adopting the guise of a beggar, to remain hidden.

Odysseus evidently arrives at his decision to conceal himself upon learning from the shade of Agamemnon, which he encounters in Hades after his consultation with Teiresias, of the extraordinary dangers that may attend a hero's homecoming (11.387–466). Agamemnon's horrific tale of betrayal and murder discloses the full dimensions of the danger that Odysseus, too, may face. Agamemnon's wife, Klytemnestra, was seduced by Aigisthos while Agamemnon fought at Troy. Although Klytemnestra initially resisted Aigisthos' advances, she finally yielded and conspired in a plot to murder her husband upon his return. Agamemnon's shade recounts the awful details to Odysseus and bitterly urges him to keep his identity a secret, not to reveal himself completely even to Penelope.

It is clear from the sequel that Odysseus takes Agamemnon's advice to heart. Odysseus grows exceedingly careful, manufacturing the most artful deceits in responding to questions concerning his whereabouts. In his first encounter with strangers (the Phaiakians) after conversing with Agamemnon, Odysseus is extraordinarily reticent and evasive concerning his true identity—notwithstanding the remarkable hospitality being extended him (cf. 7.237–39; with 8.28, 8.548–55, and 9.19–21; see also 9.504–5). A short time later, after his hosts have graciously conducted him to Ithaka, Odysseus meets the goddess Athena in the guise of a young shepherd. When asked who he is, Odysseus again dissembles, concealing his identity to gain an advantage in information (13.375–97). Athena, who has kept her distance from Odysseus since his sacrilege at Troy, at last reveals herself and, as we have seen, praises Odysseus' good judgment in keeping himself concealed.

Athena brings Odysseus up to date on the situation in Ithaka. Suitors for the hand of Penelope have established themselves in Odysseus' home and recklessly consume his property. Odysseus, of course, is put in mind immediately of Agamamnon, whose fate he is determined not to share. Instead, he resolves to keep his identity hidden. He poses as an old beggar, returns secretly to his palace, and, with the advantage of surprise (and a little help from his son Telemachos and two loyal herdsmen), kills all 108 suitors. Odysseus' mission is one of the most thrilling in all literature, owing largely to the overwhelming odds he faces and, hence, to the nearly unendurable suspense that attends every stage of his operation. The element of surprise, and thus secrecy, is absolutely imperative for his success. Clearly, then Odysseus' heroic return to home and throne depends on his willingness to conceal his identity. The good sense of his decision to do so is nearly self-evident, though it should be noted that

Table 2. Odysseus' Rational Choice

| | Circumstances | |
|---|---|---|
| Strategies | Penelope faithful | Penelope unfaithful |
| Remain concealed | Maintains element of surprise | Maintains element of surprise |
| Reveal identity | Loses element of surprise | Betrayal and murder |

such self-effacing concealment is at odds with the customary behavior of homeric heroes. Table 2, constructed exclusively from alternatives and consequences described in the text, indicates the rational superiority of Odysseus' choice.

It is clear that Odysseus' best choice is to conceal his identity. Whether Penelope remains faithful or is unfaithful, Odysseus will better maintain the crucial element of surprise by not divulging his whereabouts. In the event that Penelope has betrayed him, Odysseus can expect to suffer the same fate as Agamemnon if he announces his presence; but he can guard against this by concealing his identity rather than walking directly into the suitors' trap as Agamemnon did (11.409-15). Of course, it is most likely that Penelope has remained faithful (11.444-46), so one might suppose that, by adopting a strategy of concealment, Odysseus suffers in forestalling his reunion with a faithful and beloved wife; but this supposition underestimates the mortal danger posed to Odysseus by the suitors, for they are prepared to murder Odysseus if he should turn up, even with no encouragement from Penelope—a fact Odysseus correctly perceives (2.246-51; 17.561-65). In view of this danger, Odysseus is better off keeping his identity a secret even if Penelope remains faithful. It follows that Odysseus' decision to conceal his identity, like that of the crewmen to slaughter the sacred cattle, is the dominant strategy.[8]

It is evident, then, that Odysseus' decision is logically identical to that of his shipmates. Nevertheless, while Odysseus is praised for his good judgment in making his choice, the crewmen are condemned for theirs. On strictly rational grounds, however, it is evident that the crewmen's decision is as legitimate as that of Odysseus. What, then, is the basis of Homer's divergent judgments of rational choice? In what, exactly, does the crewmen's recklessness consist? If we return to the poem's opening, we shall discover that it is precisely the meaning of such recklessness that occupies Homer's attention immediately upon pronouncing his invocation.

## Reckless Commensuration and the Limits of Reason

The *Odyssey* opens, as we have noticed, with an implicit comparison of Odysseus with his reckless crewmen and an invocation for divine assistance in expounding this contrast. In response to this invocation, the poem's scene shifts immediately to Olympus, the home of the gods. Presumably, Homer's Muse can best answer his prayer by beginning there. On Olympus we find Zeus about to speak, as he has been reflecting upon the life of Aigisthos, Klytemnestra's seducer and Agamemnon's murderer. In his discourse on Aigisthos, the first speech delivered in the *Odyssey*, Zeus reveals the meaning of recklessness. Beginning with the story of Aigisthos, we learn that rational choice is no safeguard but can even be the instrument of recklessness.

It is with Aigisthos in mind that Zeus

formulates his renowned theodicy. Zeus holds that it is not the gods who are to blame if mortals suffer beyond their lot; on the contrary, mortals have only themselves to blame or rather "their own recklessness" (*sphêisin atasthaliêisin*, 1.34; cf. 1.7). Aigisthos is the object lesson in Zeus's instruction in the meaning of recklessness. Zeus observes that Aigisthos had been warned in advance of the vengeance of Orestes, Agamemnon's son, if he should seduce Klytemnestra and slay Agamemnon. Zeus emphasizes that Aigisthos *knew* (*eidôs*, 1.37) that he would pay the penalty for his deeds, yet he did them anyway. Evidently, the sheer intensity of Aigisthos' desire to supplant Agamemnon is sufficient to settle his choice to commit adultery and murder. Even the certain vengeance of Orestes does not outweigh his satisfaction in stealing Agamemnon's wife and throne.

We recoil in horror at hearing of Aigisthos' crimes. We have not, however, sufficiently understood the significance of this reaction of horror until we have come to terms with the fundamental rationality of the crime that incites it. Homer's judgment that such rational decisions are nevertheless utterly reckless points the way. So we must explore further the character of Aigisthos' rational choice to break the laws proscribing adultery and regicide. In this connection it is helpful to note that Zeus's discussion of Aigisthos anticipates in some important ways the approach of recent studies of criminal behavior pioneered by the economist Gary Becker (1968). Like Zeus, Becker does not acknowledge any incompatibility between criminal behavior and rationality. Moreover, the notion underlying Becker's analysis, that a legal sanction essentially establishes a *price* for a given sort of misbehavior, isolates precisely the recklessness inherent in the *criminal*'s conception of law as conceived in the *Odyssey*.[9]

On Homer's account, Aigisthos' rationalistic recklessness consists in his reckoning with Zeus's law as if it were merely a price tag. Aigisthos elects to murder Agamemnon and marry Klytemnestra because his desire to supplant Agamemnon is so great that even Orestes' divinely mandated vengeance is not sufficient to deter him. Presumably, Aigisthos knows better than anyone how much enjoyment sitting on the throne of Mycenae will bring him and what the vengeance of Orestes will cost. Thus, his calculation that the benefits of adultery and murder outweigh the costs is rational, to be sure. But his recklessness, in the poet's view, I suggest, consists in his treatment of Zeus's prohibition *in terms that even permit such commensuration*. Fundamental to this contention is an understanding that legal sanctions do not establish a price for the commission of proscribed behavior. They rather manifest, in this case, Zeus's unconditional condemnation of such conduct. A penalty for infractions exists to direct attention to this condemnation, not to invite deliberation as to its relative cost compared to that of unsatisfied and illicit desires (Berns 1979; Feinberg 1965). Ultimately, the respect that such laws properly command does not derive from any possible aggregation of carrots and sticks. It derives rather from the sacred task that is the highest and essential function of law and punishment, namely education.

In view of the educational function of law—where *education* is understood as the proper cultivation of the human soul—it is evident that a proscriptive sanction is not adequately reckoned as a price. In its deepest signification a sanction testifies to the existence of the sacred. The sacred is, above all, that which commands respect on its own terms, not in virtue of comparison or analogy with something else. By its very nature, the sacred *defies* commensuration.[10] Ultimately, the privileged status of the educational dimension of statecraft derives from the sanctity of the human soul, which it aims to improve. It is an insight into the sanctity of the soul, as we shall see, that proves to be Odysseus' major learning experience and the trigger of his change of heart; but for our immediate purposes, the sanctity of the soul is most readily illuminated by a familiar biblical passage: "What will it profit a man," Jesus asks, "if he should gain the whole world but forfeit his soul?" I take it that this question is a rhetorical one. The point is that *nothing* in the entire world can compensate one for, or make one indifferent toward, the loss of his soul. Thus no worldly thing can "take the measure" of man's soul. The soul, we may say, defies commensuration with worldly things.

Rationalism, however, upholds the limitless possibility of commensuration because it presumes the ultimate hegemony of reason in the pursuit of wisdom. If everything were commensurable, reason would indeed be sovereign in discernment. But Homer's *Odyssey* indicates that even a perfectly operating rational faculty, as in the cases of Aigisthos and of Odysseus' shipmates, fails utterly at discerning what is necessary for wisdom, namely the fundamental difference between the sacred and the profane. Where reason accepts such a distinction, it does so on the authority of something higher than reason. Therefore, the consistent rationalist cannot accept (even axiomatically) a sacred "value" as given. In propounding a universal hegemony for reason, the rationalist juggernaut inescapably collides with the sacred. The strategy of the rationalists' thrust against this obstruction is simply to ignore those characteristics of the sacred that are not expressible in terms common with the principal objects of their interest. This is accomplished most easily in the case of the law by disregarding the educational significance of penalties and interpreting them simply as prices. As the rationalist sees it, laws do not educate, they merely regulate. Nothing is sacred, everything has its price.

Along with Aigisthos, Eurylochos and his mates are the unreconstructable rationalists in Homer's *Odyssey*. Eurylochos' speech to the crewmen shamelessly displays an utter indifference to the sanctity of Helios' cattle. He treats the divine sanction proscribing Helios' cattle as though it were simply a signal of the price charged by the gods for Thrinakian roast beef. With Eurylochos showing the way, the crewmen thus reckon the relative costs and benefits of their crime with faultless logic. As we have seen, they determine that it is best to slaughter the cattle because even in the worst case, they heartily prefer a quick death at sea to slow starvation. Because this reckoning is fundamentally heedless of the educational significance of punitive sanctions, I suggest, Homer condemns the men as "utterly reckless."

Homer condemns the crewmen's rational choice not because it is rational, then, but because it is *rationalistic*. The crewmen carelessly transgress the limits imposed by the sacred upon rational commensuration. We should carefully note that in condemning such rationalistic excess, Homer proves to be not a misologist but a friend to reason. To appreciate Homer's stance, one must understand that rationalism is not an alien influence that infects reason with some foreign contagion. Rationalism is rather a form of licentiousness; it works its influence from within. Perhaps this can be clarified most effectively with the aid of a political analogy. Rationalism is to reason as majority tyranny is to democracy. Friends of reason, like James Madison's (*Federalist* No. 10) "friend of popular government," must take care to guard especially against the vicious propensities that inhere in their favorites. Just as we learn from the framers of the U.S. Constitution that the fluorishing of democracy depends upon institutions that *limit* the rule of majorities (Diamond 1959), so too we learn from Homer that the flourishing of reason into wisdom requires a recognition of *its* limits. This recognition is implicit in respect for the sacred, such as that which determines Odysseus' refusal to join his crewmen in their rationalistic smorgasbord. It remains, of course, to consider how it is that Odysseus develops this life-enhancing reverence while his crewmen are condemned to a self-destructive rationalism.

## The Etiology of Rationalism and the Roots of Reverence

Careful consideration of Eurylochos' speech on Thrinakia will reveal the underlying source of the crewmen's rationalism. The key premise in Eurylochos' argument, which facilitates the commensuration of death by starvation and death by divine punishment, is that "all deaths are hateful to wretched mortals" (*pantes men stygeroi thanatoi deiloisi brotoisi*, 12.341). This is a surprising contention. One supposes that death might sometimes appear to be a blessed release rather than something indiscriminately hateful, particularly to a "wretch." But Eurylochos maintains that *all* deaths are hateful. This is because, in his view, wretchedness is not the cause of man's indiscriminate orientation toward death, but (as we shall see) *its effect*. Eurylochos' contention, by itself, still appears to be a logical non sequitur, however. Why then is it so persuasive to his audience? The answer is that Eurylochos' argument is an enthymeme, not simply a syllogism. In other words, its cogency depends upon a suppressed premise that his auditors supply on their own. Only if we take it that human beings are haters of mortality in itself, does it follow that they will be *indiscriminate* haters of death. The influence of this premise in making possible Eurylochos' subsequent commensuration is decisive. For a hatred of death per se will obscure the qualitative differences among various ways one may come to die. Then all that remains is to reckon the length of life and how much pain and pleasure is felt during its span. Questions of "how long" and "how much" obviously admit a common measure; only commensurables are involved. Hence, as we have seen, Eurylochos' appeal to his shipmates rests simply on the superiority of a quick death to a slow death.

The obscurantist tendencies of the crewmen's hatred of mortality engender a mistaken conviction of universal commensurability and, consequently, of the sovereignty of reason. The hatred of mortality as such conceals utterly the difference between the sacred and the profane, the noble and the base. It is this lack of discrimination that, in turn, truly accounts for the wretchedness Eurylochos attributes to mortals. The root meaning of Eurylochos' word for "wretched," *deiloisi*, literally denotes "cowardice." The wretchedness of the coward consists, above all, in a blindness to the qualitative difference between what is and what is not worth the risking of one's life. The hatred of mortality as the cause of cowardly wretchedness and its consequent lack of discrimination is indeed the great leveler. Once it has thus accomplished its work, the horizon of reason appears limitless. If we wish to account for the crewmen's rationalism, then, we must consider the genesis of their hatred of mortality. While Eurylochos and his mates evidently suppose that this hatred is simply in the nature of man, Homer suggests a somewhat different and certainly more complex view. As the poet shows, all men are not haters of mortality. Odysseus stands out as an exception.

Unlike his crewmen, Odysseus is not a hater of mortality. Nevertheless, neither is he altogether dissimilar to his mates. Odysseus, too, has been guilty of recklessness, as we have seen (cf. 18.139 with 1.1–2; 10.437). Odysseus' recklessness derives from a source Homer recognizes as deep within the human soul, namely the *thymotic*, or "hearty," longing to accomplish great things, to win reputation and glory. Among the crewmen, a rather ordinary bunch unable to vent themselves heroically, this spirited passion has soured into a bitter and self-destructive hatred of mortality. It is not surprising that such men would blame and grow to hate their own mortality. For it is mortality that sets the ineluctable limits that finally prevent mediocrities from accomplishing great feats—if only because time runs out. In Odysseus, by contrast, this hearty passion issues first of all in a typically heroic (one could say, "Achillean") competitiveness. The path of heroic competitiveness has its own shortcomings, of course. By itself, such competitiveness knows no bounds. In its purest form, it is directed against the gods themselves, as the hero rivals the immortals in his own attainment of immortal glory. Thus, Odysseus recklessly ravages Troy's sacred citadel, the image of Olympus. He gloats, recklessly, over his defeat of Poseidon's son, Polyphemos the cyclops. Nevertheless, what distinguishes Odysseus from the likes of Achilles, Diomedes, and Ajax—to say nothing of his crewmen—is that he overcomes this reckless heroic competitiveness before it destroys him.

The poet underscores the importance of this development in a remarkable manner. Our first direct glimpse of Odysseus in the poem occurs as he is *refusing* the goddess Kalypso's offer of immortality (1.58-59, 5.206-24). In fact, we are told that Odysseus "longs to die" (*thaneein himeiretai,* 1.59), so great is his desire to see hearth smoke leaping from his native Ithaka once again. From this point the narration of Odysseus' story retraces his steps by way of flashback in an effort, I suggest, to account for the remarkable choice he has made. Odysseus' reverent self-control on Thrinakia is presented as the acid test of the change of heart that ultimately makes possible his refusal of Kalypso's offer. If we wish to learn what first triggered Odysseus' change of heart, his new appreciation of death and mortality, it is necessary above all to ponder the significance of his journey to the land of the dead. It is there that Odysseus encounters the shades of his comrades at Troy, the celebrated heroes Agamemnon and Achilles.

We have already considered the influence of Odysseus' interview with the shade of Agamemnon upon his decision to forego heroic self-aggrandizement and conceal his identity. But it is Achilles, more than anyone, who may be considered the expert witness on human *thymos* and the heroic longing for immortality. We expect that Odysseus' conversation with him will be particularly enlightening. Achilles appears just as Odysseus and Agamemnon are concluding their tearful exchange of tales, Agamemnon of his betrayal and murder at the hands of Aigisthos and Klytemnestra, and Odysseus of his harrowing experiences at sea (11.465-67). Achilles, who is weeping himself, approaches and asks how Odysseus dares while still living to come to Hades. In response, Odysseus recounts to Achilles the purpose of his mission and bemoans the evils that seem perpetually to obstruct his homecoming. Then he addresses himself to Achilles' condition. Though deeply moved by Agamemnon's suffering, Odysseus finds it most surprising that the illustrious Achilles should have any reason to lament. Odysseus contrasts his own sufferings with what he maintains is Achilles' unprecedented blessedness. What is death, wonders Odysseus, compared to the glory of Achilles? Achilles, Odysseus declares, is the happiest (*makartatos,* 11.483) man of all time—in life he was honored "as an equal to the gods," and now he rules magnificently over all the dead. In response, Achilles crisply orders Odysseus not to make light of death. In one of most famous passages in the *Odyssey,* Achilles bitterly proclaims his preference for mortal life—even the least glorious life imaginable as the slave of a nobody—over being the illustrious monarch of the underworld (11.489–91). To rule the underworld is the station of a god, yet Achilles suggests that Odysseus is badly mistaken in supposing that the exchange of mortal life is a good trade for such an apotheosis. The greatest Achaian hero, who vied with the gods in his wrath and in his longing for glory, evidently regrets his success. It would be an understatement to say that the effect of this speech on Odysseus is considerable.

We will entirely miss the significance of this speech for Odysseus, however, if we merely look to the most famous commentary upon it for its interpretation. In Plato's *Republic,* Socrates does not approach the interpretation of Achilles' speech from Odysseus' perspective but from the standpoint of young, impressionable cadets. To be sure, Achilles' speech could sap the courage of callow youth (Plato 1972, 386a6–c7; cf. 378d5–e1). Someone young in character and understanding might hastily infer, on the basis of Achilles' preference for slavery to death, that no cause warrants the risk of one's life. But surely it would be a great error to mistake Odysseus for a callow youth. His reception of Achilles' speech is different. If we attempt to interpret Achilles' speech as Odysseus understands it, we must begin by squaring it with what Odysseus knows of Achilles' character. Above all, Achilles is unrepoachably courageous. So Odysseus, who fought beside Achilles for 10 years at Troy, would know better than to interpret his speech as the counsel of a coward. Next, we must keep in mind that Achilles intends his speech as a rejoinder to Odysseus' unwarranted congratulations on his supposed blessedness. Odysseus makes light of death because he supposes Achilles' godlike immortality is worth more than mortal life. Achilles obviously disagrees. But if Achilles' preference for even the meanest mortal life over his godlike station in Hades is unintelligible as the counsel of a coward, how can it be understood? The key to answering this question, it seems to me, is the recognition that Achilles' preference reflects the *sanctity* of mortal life, its fundamental incommensurability with even a godlike immortality. One can reconcile Achilles' speech and deeds only by recognizing that life is sacred and thus that his preference of the mortal over the immortal is lexicographic.[11]

Odysseus finds a great truth in Achilles bitterness. Achilles represents better than anyone man's heroic longing to *be* someone. Immortality seems the way to be because being would appear to entail permanence. This is certainly the case with the gods, whose being is distinguished above all by their immortality. For mortal man, though, there is a difference. A mortal's being consists in what Homer calls *psychê,* which means both life and soul. We have no assurance, however, that *psychê,* even as soul, is immortal; nor is even the possession of *psychê* to be taken for granted. In the Homeric perspective, man must "win his soul" (*arnymenos hên te psychên,* 1.5). Only then will he come into his own. For a human being, striving for immortality is simply beside the point (cf. Matthew 16:25). Profiting from Achilles' experience, Odysseus learns that mortal men become godlike at the cost of their own souls. Achilles' shade is not a true soul but merely a "witless and exhausted phantom" (11.473–76; 10.492–95). In his attempt to rival the gods, Achilles lost the most divine thing in himself. This is the paradoxical truth beneath Achilles' bitter and easily misconstrued denunciation of his position in Hades. Of course it is too late for Achilles to benefit from this lesson, but it is not too late for Odysseus.

Beginning with his refusal to participate in his crewmen's slaughter of Helios' sacred cattle, Odysseus resists on his own repeated temptations to treat mortals and immortals as commensurable. He consistently rebuts any comparisons between himself and the gods (e.g., 5.208–24, 7.208–21, 16.186–91). In his choice of women, Odysseus prefers Penelope to the goddess Kalypso—despite the fact that on any common dimension of comparison Kalypso is superior (5.211–18). He rejects Kalypso's offer of immortality without a second thought. Where, earlier, Odysseus had been inclined to praise and even to envy Achilles for being esteemed equal to the gods, he now draws a strict line in his own ambitions. He will vie only with mortals (8.221–25). Rivalry with the gods, Odysseus comes to understand, is merely reckless commensuration in heroic guise. Death, the most tangible dividing line between mortals and immortals, becomes something for which Odysseus urges respect. He refuses to glory over the dead bodies of even his most hated enemies, the reckless suitors of Penelope; he declares that to do so would be "unholy" (22.410–13). Where his crewmen's hatred of mortality issues in the rationalistic violation of the sacred cattle, Odysseus' appreciation of mortal life disposes him to respect *especially* the cattle of Helios. For it is Helios, the sun god, who is sovereign over the days and seasons that measure the limits of our mortality (Flaumenhaft 1982). Genuine reverence to the sun god is to be found in a respectful appreciation of these limits rather than in the belief in universal enlightenment.

Homer does not permit Odysseus to resume the throne of Ithaka without a final recognition of his newly developed reverence, so important it is to his claim to heroic status. The political consummation of Odysseus' return to Ithaka, a settlement between his house and the families of the suitors whom he has killed, depends upon his respecting certain limits to the satisfaction of vengeance and the execution of justice. In the poem's final battle, which is incited by the suitors' angry relations, Odysseus soon wins the upper hand. If Odysseus were to press his advantage to the point of decimating the suitors' relatives, however, a peaceful settlement would become untenable. A protracted and bloody vendetta would result instead. All the same, justice would seem to authorize the punishment of those who have attempted to murder Odysseus and who for three years stood by without a word as their own sons and brothers recklessly abused his household. But at precisely the point when Odysseus' advantage is clear, Athena intervenes. Athena commands Odysseus in the name of Zeus to yield. Homer notes that Odysseus obeyed but did not *merely* obey—he rejoiced in his heart (*epieitheto, chaire de thymôi,* 24.545). With this statement Homer indicates that Odysseus' change of heart is complete. For his rejoicing in Zeus's command is not a merely ritual piety before a more powerful god. Odysseus' joy reveals that his heart, his *thymos,* is in full accord with the delimitation of commensuration, even in the difficult case when such commensuration is in the service of distributive justice. Justice, as should already be evident, is not the highest virtue in the *Odyssey.* On the contrary, it is thanks to his reverence that the fierce spirit of Odysseus in the end fully internalizes this gentle lesson in moderation. It is by means of his comprehensive change of heart that Odysseus comes into his own, at last winning his soul. This achievement, which even Achilles cannot match, establishes the heroic superiority of Odysseus. With this the *Odyssey* comes to a close.

## Conclusion

We began by noting that the study of Homer has fallen into a state of neglect and even disrepute among modern students of politics and by suggesting that perhaps, upon consideration, the *Odyssey* might be found to be both relevant and beneficent in view of the special needs of our own polity. This consideration focused on Odysseus' adventure on the island of Thrinakia, where he distinguishes himself most decisively from his recklessly rationalistic shipmates. At first glance, the basis of this distinction appeared untenable. Homer condemns the crewmen for their recklessness in electing a strategy that is logically identical to a subsequent strategy of Odysseus that earns him praise for his good judgment. We resisted the temptation to attribute this discrepancy to a host of external factors and attempted instead to look further within the poem to discover the basis upon which the poet would dare to condemn the crewmen's undoubtedly rational choice. Finally, we sustained Homer's charge of recklessness and attributed it to the crewmen's heedlessness of the limits of rational commensuration. These limits are dictated by the sacred, which warrants respect in itself, not by virtue of analogy or comparison with anything else. In accordance with their rationalistic presumption of the boundless powers of reason, however, the crewmen treat the divine sanction of Helios' cattle as if it were a mere price tag.

Odysseus, by contrast, does not violate the sacred cattle. His intelligence (like Achilles' courage) is beyond reproach; but Homer's elevation of Odysseus to heroic status does not simply rest upon that intelligence. Odysseus' heroic excellence, which truly sets him above the other Achaian kings, consists in his combination of godliness and resourceful intelligence. Odysseus comes to accept this reverential constraint on his intelligence as he learns that human beings, however they may long for immortality, cannot rival the gods. Such a competition is out of the question because man simply cannot "take the measure" of the gods. As Odysseus learns from Achilles, mortal man becomes more godlike at the cost of his own soul—the most divine thing in him. From this paradoxical insight into the fundamental incommensurability of men and gods, Odysseus gains an appreciation of the excellence that is specific to human beings. Odysseus' appreciation of human mortality contrasts most sharply with the hatred of mortality of his cowardly shipmates. While their hatred of mortality urges them on to recklessness, Odysseus' appreciation of human mortality accompanies a sobering divination of the limits that properly constrain human reason. By virtue of this change of heart, Odysseus *breaks with* the customary behavior of the "typical hero." Odysseus' reverence, therefore, should not be confused with a merely blind observance of custom in whatever guise that custom may present itself. On the contrary, Odysseus' reverence is above all inclined to political moderation, as the conclusion of the poem indicates.

Homer's criticism of rationalism could be studied with profit at any time. But perhaps nowhere does this lesson have greater *political* importance than in a modern commercial republic like our own. Harbored within this ordinarily mild polity is a dangerous propensity that threatens to undermine the very liberty that is its hallmark and precious heritage. Commercialism, taken to excess, holds nothing sacred, not even the moral and political principles that make free trade, to say nothing of our other freedoms, possible. To the unbridled commercialist, everything has its price. This rationalistic propensity of the commercial republic provides its adversaries across the range of the political spectrum with "fruitful topics" for their "specious declamations." Solzhenitsyn joins Lenin, if in little else, in scorn for capitalists who one day will sell the rope their enemies will use to hang them. In this regard, it is most fitting and highly satisfying that Odysseus, whom some have disparaged as "the acquisitive hero," provides a choice lesson for friends of the commercial republic.

The unbounded pursuit of commercial maximization is, after all, nothing if not a variety of rationalism. Like Odysseus' crewmen, Lenin's hypothetical capitalist reduces fundamental constitutional or legal principles to mere price tags. But the substance of such fundamental laws and rights is formulated with a view to the human soul and its proper cultivation; to disregard their sanctity is an error Homer reveals as the height of recklessness. In light of Homer's *Odyssey*, we see more clearly the significance of Strauss's arguments on the need to remain open to the claims of revelation and Hayek's arguments against rule utilitarianism and the presuppositions of "Posnerian judges." We also gain a deeper appreciation for the wisdom of our founders. Natural rights, they proclaimed, are "unalienable" and "self-evident"—which is to say that their authority is not dependent upon rational demonstration and that they are not to be rearranged merely to promote "social wealth." The "genius of American politics" lies not in a dedication to rationally demonstrable first principles but rather in a solemn respect for the "givenness" of common-law liberties and the equal rights with which we are endowed by our creator (see Boorstin 1953, chap. 1). In maintaining the "self-evidence" of natural rights, the founders disagreed decisively with Thomas Hobbes, himself an expounder of a doctrine of natural rights. Hobbes, however, does not settle for the self-evidence of natural rights. Instead, he presents an impressively rationalistic derivation of these rights. We must not fail to note, however, that this derivation culminates in the institution of an authoritarian state, the Leviathan. Absolutism, the antithesis of liberty, would seem to be the consequence of rationalism in politics. Friends of the commercial republic might rightly conclude, then, that it is not the emulous zeal of impressionable readers of Homer that most endangers their favored political order. A much more pressing threat is posed by the irreverent conviction of the hegemony of reason, to which commercialism itself inclines us. If, in opposition to this inclination, we recommend that such moderate and moderating first principles as those to which our nation is dedicated be respected as sacred, we are following a trail blazed by our own founding fathers, but charted originally, I suggest, by the poet of the *Odyssey*.

## Notes

1. Is reason, as we understand it, sufficiently recognizable in the understanding of the ancients to accommodate the intelligibility of a claim designating Homer as a critic of rationalism? This is a complex issue, but one that can perhaps be addressed sufficiently for present purposes. The semantic range of reason (*logos*) among the ancients is best indicated by the denotation of the word itself. In Greek *logos* literally means a reckoning or giving of accounts, principally by ratio or analogy. At the heart of this complex of meanings is the notion of commensuration. Thus an "irrational" number—the usage has survived intact from the earliest days of ancient mathematics—is so termed precisely because of its incommensurability. One is not able to give an account of such a number in terms of other numbers, that is, as a ratio. Whatever their other differences, ancients and moderns alike acknowledge commensuration as the characteristic modus operandi of reason. It is against the limitless hegemony of reason thus conceived that I understand Homer's attack in the *Odyssey* to be directed.

2. Thanks largely to Austin's (1975) contribution, it is again admissible in academic circles to speak of Homer's intention. Let us grant to the "analytic" and "oralist" schools of Homeric interpretation that the manuscript tradition is imperfect and that the most ancient bards performed without benefit of a written text. But let us also note that those who object to the presumption of compositional integrity in the *Odyssey* typically rest their case on the dubious assumption that textual discrepancy necessarily implies a lack of design. As I endeavor to show in this essay, such discrepancies may well play an essential role in the poet's presentation of even his most important teachings.

3. David Davies's (1985) perceptive account of the remarkable delicacy of judgment displayed by Odysseus in wresting and then returning Agamemnon's scepter (*Iliad* 2) leaves no doubt as to the poet's respect for the excellence of Odysseus' mind in the

*Iliad.* Stanford (1985, 74–76) holds that all of Odysseus' "untypical" excellences, except one, are manifest in both poems. He maintains that only the intellectual curiosity of Odysseus appears uniquely in the *Odyssey.* In my view, however, Odysseus' desire to see peoples and places along his route back to Ithaka does not issue so much from a novel love of learning as from his accustomed swashbuckling adventurism. It is significant that once he is cured of these swashbuckling tendencies, Odysseus is interested in seeing one sight alone—hearth smoke rising from his native Ithaka (1.58–59). This *new* concern for hearth and home is evidence of the dawn of reverence in Odysseus.

4. Careful attention to the epithets used of Odysseus will uncover an interesting pattern that further substantiates this change. Prior to the present time of the narration of the *Odyssey,* particularly during the extended flashback of books 9 through 12, Odysseus' most common epithets consistently refer to his intelligence (*polymêtis, polymêchan';* see Dunbar's *Concordance to the Odyssey*). But from the time of the poem's narration, which commences with Odysseus' refusal of Kalypso's offer of immortality, no epithet is used of Odysseus more frequently than *dios,* or godly. Odysseus, as we shall see, is adamant from this time on in his opposition to any suggestion of comparison or rivalry with the gods. It follows that those translators who render *dios* as "godlike"—implying a blurring of the distinction between Odysseus and the immortals—have it exactly wrong. Like our word *godly, dios* evidently has another significance besides godlike. A godly man is one "devoutly observant of the laws of god" (*Oxford English Dictionary* 4:273).

5. My translations in this essay are based on W. B. Stanford's edition of the *Odyssey* (Macmillan & Co., London, 1965). Citations are keyed to book and line number.

6. It is worthwhile to consult Jon Elster's (1979) analysis of Odysseus "strategy of precommitment" in dealing with the Sirens' temptation. I would supplement Elster's account with the observation that Odysseus' strategy is exercised against a desire for universal knowledge, which the Sirens expressly promise to satisfy (12.184–91; also cf. 12.49 with 12.160). In this sense, Odysseus' strategy can be understood as a preliminary means for resisting the Siren song of *rationalism.* The moderation ultimately necessary in such a case, however, would involve not merely an external control of the appetites but a change of heart that itself recognizes the peculiar limits of a human wisdom.

7. Except, perhaps, in the rare case when this decision rule appears to conflict with the maximization of expected utility, as contrived in Newcomb's Problem. The clearest statement of the controversy surrounding this problem, its political relevance, and its resolution—by recasting its gaming elements into independent decisions—is presented by Steven J. Brams (1976, 194–203). In the present case, we may easily point to evidence of such independence (see, e.g., 12.382–87). But it is also clear that the two decision rules converge here anyway because the men's preferences across the alternatives are quite intense. Satisfying one's hunger with impunity is much preferred to starvation, as goes without saying. But even the preference for a quick death to a slow death is described by Eurylochos as "hearty" (*apo thymon,* 12.350).

8. One might, in an expanded matrix, consider Odysseus' options to reveal his identity to particular individuals. The dynamics driving his decision toward concealment would be the same, however, even in the difficult case of Penelope herself. Granting that Penelope is faithful, Odysseus still fears that in her joy at discovering he is home she will alarm the suitors and inadvertently cause his destruction (13.192–93, 16.301–3, 19.476–86). Of course, Odysseus eventually does make himself known to Telemachos and to Eumaios and Philoitios. In each of these cases, Odysseus chooses to dissemble until he is satisfied that the consequences of revelation, whatever Penelope's faithfulness, will not lose him the element of surprise (see, e.g., 16.151–68, 454–49; 21. 192–229).

9. Because the economist's interest in formulating a "production function" for crime compels him to adopt the perspective of the criminal, it is only fair to note that this is not *necessarily* Becker's own understanding of sanctions. Still, it is significant that it is the poet, not the economist, who can go on to reveal the rationalism implicit in such a conception as itself a root cause of criminal recklessness.

10. The defiance of commensuration essential to the sacred is further substantiated by the comic consequences of calling it into question. Henny Youngman tells the story of a salesman whose customer innocently asks "How's your wife?" "Compared to what?" is the salesman's snappy response. The razor's edge of Youngman's joke results from his opposite and precisely balanced strokes of rationalism and reverence, the former evident in the salesman's proverbially exclusive orientation to "bottom-line" comparisons, the latter manifest in our expectations concerning the sanctity of marriage. Spouses, after all, are loved in themselves—not merely by comparison with others.

11. A lexicographic order ranks alternatives according to a primary criterion without regard to their rank on a secondary criterion. In the case of a tie, the tied alternatives are ranked according to a second criterion without regard to their rank on a third, and so on—the way words are ordered in a dictionary. Achilles' preference, like all lexicographic orderings, does not violate transitivity. Lexicographic preferences are rational in this purely ordinal sense, though they preclude rational comparisons in the more meaningful sense of reason as commensuration. Riker and Ordeshook (1973, 43),

who observe that the *fundamental* aims of politics typically resist commensuration, are instructive in this regard. These authors, to their credit, accept the existence of such incommensurability, which they represent with lexicographic preferences, as defining limits to their rational-choice theory of politics. See also the excellent discussion of Elster (1979, 125), who rightly notes that the lexicographic character of such incommensurables is best understood as constituting "constraints on decision-making rather than as criteria for decision-making."

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# THE EFFECT OF APPROVAL BALLOTING ON STRATEGIC VOTING UNDER ALTERNATIVE DECISION RULES

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*Voting systems combine balloting methods with decision rules or procedures. Most analyses of approval voting (a balloting method) assume it will be combined with plurality rule but advocates often urge its use with more complex procedures. Because much of the case for approval balloting hinges on its encouragement of sincere voting, we ask whether it retains this advantage when combined with multistage procedures. After distinguishing five forms of sincere and insincere approval voting, we find that certain elements of multistage procedures promote departures from purely sincere strategies, including, in some instances, strictly insincere voting. However, most strategic approval voting involves truncating the approved list, including bullet-voting, which is especially likely under certain threshold rules. Coalitions also increase members' incentive to truncate. We conclude that approval balloting with plurality rule remains preferable to conventional single-vote plurality, but we urge caution and further research regarding combining approval balloting with multistage rules.*

Since its invention only about a decade ago, approval voting has attracted interest and endorsements—some highly enthusiastic—from scholars and political leaders. However, because the method has not yet been tried in many actual voting situations, claims in its favor are supported mainly by a prolific and ingenious theoretical literature. Necessarily, theorists build into models assumptions about the nature and circumstances of choice that are simpler than political reality. If prescriptions derived from theory are applied under conditions more complex than those assumed in the models, unintended consequences may ensue that could discredit the reform in all situations, even those that correspond better to the original premises.

One motive of this paper is to guard against such danger by investigating conditions under which the approval ballot may encourage undesirable patterns of voting behavior.[1] If we can identify circumstances where this new electoral medicine is unproven or contraindicated, then we can avoid overprescribing it and so protect it from falling into disrepute.

According to proponents, a major virtue of approval voting compared to conventional voting systems is its greater "honesty" in the sense that it encourages sincere voting and is less vulnerable to strategic manipulation (Brams and Fishburn 1978; 1983, chap. 2). Under the common "plurality" election system, a voter choosing among three or more candidates often faces a strategic dilemma—whether

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to vote "sincerely" for his or her most preferred candidate or "realistically" for a less-preferred candidate who has a better chance to win. Most of the presumed advantages of approval voting follow from its ability to end this "wasted-vote" problem by allowing the voters to eat their cake and have it too by approving more than one candidate.

However, as Niemi (1984) and Arrington and Brenner (1984) emphasize, approval balloting introduces a new strategic dimension by forcing the voter to decide *how many* candidates to approve. At first blush, it will seem to many people (as it does to Arrington and Brenner) that citizens who cast *more* votes will have an unfairly greater effect on the outcome. Brams and Fishburn (1983, 1984) show that this worry is based on a naive conception of voting power. The more sophisticated fear is that some voters will gain a strategic advantage by voting for *only one* candidate.

Two experiences with approval balloting help illustrate the problem. In December 1983, Pennsylvania Democratic party leaders cast approval-ballot straw votes to show their support for the eight candidates then campaigning for the presidential nomination (Nagel 1984). When academic advocates of approval voting met with party chair Edward Mezvinsky to plan this experiment, he was especially concerned about the risk that voters committed to a particular candidate would cast bullet votes, a practice Pennsylvania politicians call "plunking."[2] If nearly all participants plunked, the distinctive merits of approval voting would be negated. If, on the other hand, only one or two factions plunked, Mezvinsky felt they would gain an unfair advantage.

As it turned out, of the 201 party leaders who participated in the straw vote, 58% cast more than one approval vote. Academic observers were satisfied because nearly all the plunking conformed to theoretical predictions about rational behavior, which implied it was sincere (Nagel 1984). Nevertheless, some political leaders remained troubled that so many as 42% plunked.

A short time later, plunking again became an issue in an advisory election by a faculty unit, which decided to forward to its dean the names of all candidates receiving majority support on an approval ballot. When the votes were tallied, it became apparent that one faction had adopted a bullet-voting strategy. As a result, their favorite led by a comfortable margin. The coalition did not succeed in preventing other candidates from reaching the majority threshold, but some fell perilously close. The faculty who did not bullet-vote accused the plunkers of colluding to their own advantage at the expense of the general interest.

Why did plunking appear less innocuous in the faculty ballot than in the politicians' straw vote? There was one important procedural difference: the faculty intended to use a majority-vote cutoff, or threshold, to narrow the field before a second stage of choice (the dean's decision). Brams and Fishburn (1983, 42) have previously analyzed another kind of two-stage procedure, the runoff, in which a majority threshold must be attained in an initial vote in order to avoid a second stage. They conclude that "coupling approval voting to a runoff system . . . produces a combination that is *never* strategyproof." The faculty vote led us to ask whether this warning should be extended to other two-stage decision processes and/or to other elections involving thresholds. Our inquiry acquired added significance when we realized that two-stage choices are more prevalent than they at first appear; they include nomination procedures and processes involving proportional representation. Included in both categories is the foremost target of approval-voting advocates—the U.S. presidential nomination process.

Our purpose, then, is to investigate the sincerity of voting strategies under approval balloting when it is combined with various decision rules, especially two-stage procedures. Our paper proceeds through three stages of its own: (1) a discussion of the meaning of manipulation and honesty, in which we introduce a new, more differentiated view of sincerity in approval voting; (2) an analysis of the elements and types of voting systems; and (3) a presentation of the reasoning and results on which we base conclusions about the likelihood of each version of sincerity for each type of approval ballot voting system.

## Five Meanings of Sincerity and Insincerity

Many critics of approval balloting ignore either or both of two major results achieved by Gibbard (1973). One, which some writers overlook in making impossibly perfectionist demands, is the general proof that *all* voting systems are manipulable. More relevant here is Gibbard's demonstration that the charge of manipulability depends on a definition of honesty that must be "to some degree arbitrary" (p. 590). This is emphatically the case with approval balloting.

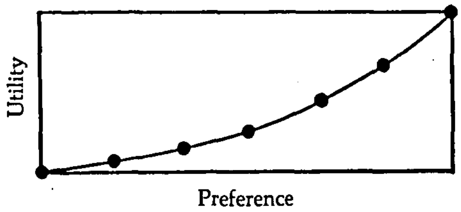
Normally, theorists equate "honesty" with the more precise concept of "sincere" voting. For approval balloting, Brams and Fishburn (1983, 29, 178) define a voting strategy (i.e., the set of candidates for whom one votes) as insincere if and only if the voter casts votes for all candidates whom the voter prefers to the least-preferred candidate for whom he or she votes. At first this definition (which we call the *no skipping* rule) seems unexceptionable, but it dubs voters "sincere" who strategically plunk, as long as they plunk for their most preferred candidate. Thus the door is open for Niemi (1984, 958) to make the seemingly paradoxical charge that "although [approval voting] may indeed promote sincere voting . . . it is neither honest, strategy proof, nor wise."

In the hope of generating less heat and more light, we propose a more complex understanding of sincerity and insincerity under approval balloting. To differentiate strategic sincerity from "honest" sincerity, we need a baseline prediction about the voting behavior of a perfectly ingenuous citizen. Fortunately, such a standard is available in the rule derived by Merrill (1979) and others (Brams and Fishburn 1983, 84–88) that an individual voter acting alone without knowledge of others' strategies maximizes his or her expected utility by voting for every candidate whose utility for that voter exceeds the average (mean) utility of all candidates. From this starting point, we proceed to five definitions for approval balloting. A voting strategy is said to *skip* if it includes a vote for any candidate without also including a vote for all candidates strictly preferred to that candidate.

1. *Pure Sincerity.* A voter's choice is *purely sincere* if he or she votes for precisely those candidates whose utility exceeds the average.

   What would pure sincerity look like in actual voting situations? If we assume that candidate utilities are symmetrically distributed, an operational substitute would have the voter approving half the candidates. This strategy also maximizes the number of distinctions the voter makes between pairs of candidates, which is equivalent to the voter's "efficacy" or power to change the outcome without taking preferences into account (Brams and Fishburn 1983). There is reason to believe, however, that the personal utilities of a voter are likely to be skewed, with the greater number of candidates bunched toward the low end of the scale. Snider (1979) obtained empirical plots of utility versus preference that approximate the curve $u = (\text{rank}/M)^2$, where $M$ is the number of

**Figure 1. Utility of Candidates versus Preference (from Lowest to Highest)**



candidates (see Figure 1). For this distribution of utilities, less than half the candidates lie above the mean utility cut point, which suggests that even a purely sincere strategy usually involves voting for fewer than half the candidates. This assumption is consistent with behavior in the Pennsylvania straw vote (Nagel 1984) and in a number of approval-ballot polls (Brams and Sharrard 1979; Brams et al. 1981; Kellett and Mott 1977).

2. *Expansive Sincerity.* A choice is *expansively sincere* if the voter votes for more candidates than prescribed by pure sincerity without skipping.[3]
3. *Restrictive Sincerity.* A choice is *restrictively sincere* if the voter votes for fewer candidates than under pure sincerity without skipping. We also call a restrictively sincere strategy *truncation;* the limiting case is plunking for one's most preferred candidate.
4. *Weak Insincerity.* A choice is *weakly insincere* if it involves a skip but includes a vote for the voter's first choice.
5. *Strong Insincerity.* A choice is *strongly insincere* if the voter votes for at least one candidate but does not vote for his or her most preferred candidate. In keeping with the seriousness of this offense, we call it *decapitation.* Note that the concept of strong insincerity, unlike the other four, applies to conventional as well as approval balloting. It is, in fact, the type of insincerity that approval voting was invented to avoid.

## Voting Systems

Nearly everyone who writes about "approval voting" compares it with "the existing plurality method" as if the two were in the same conceptual category. They are not.

Any voting system consists of two components: a *balloting method* and a *decision procedure* or *rule.* The plurality principle, as traditionally understood, is not a balloting method but a decision rule—one that assigns the $K$ offices at stake in an election to the first $K$ candidates ranked in order of votes received, without regard to thresholds. (Henceforth, to avoid undue complexity, we consider only the case where $K = 1$.)[4] Traditionally, plurality rule has been contrasted to the majority principle, which requires that a victorious candidate must receive more than half the votes cast. In the recent literature, the conflation of balloting method and decision rule through the phrase "plurality method" has become a source of confusion not just in language but also in thinking. We therefore urge that others join us in adopting the following verbal conventions:

1. *Voting System.* A *voting system* is the combination of a *balloting method* (which specifies how votes may be cast) and a *decision rule* or *procedure* (which specifies how the ballots are aggregated to determine the winner or winners). Voting systems should be designated by a combination of two words (or phrases or abbreviations), the first indicating the balloting method and the second, the decision rule.
2. *Approval Ballot (AB).* The *approval ballot* allows the voter to cast one vote per candidate for up to $M$ candidates,

where $M$ is the number of candidates seeking office.[5]

3. *Single-vote Ballot (SVB).* The *single-vote ballot* allows the voter to cast one vote only (when $K = 1$).[6]
4. *Approval Plurality (AP). Approval plurality* is a voting system that combines approval balloting with the plurality decision rule. That is, the candidate who receives the most approval votes wins, regardless of whether he or she attains a majority or any other threshold.
5. *Single-Vote Plurality (SVP). Single-vote plurality* is a voting system that combines single-vote balloting with the plurality decision rule. This is what other writers refer to as "the plurality method," most commonly used in U.S. elections.

Until now, nearly all theoretical analyses of approval voting have compared approval plurality with single-vote plurality. This body of research has established a number of cogent arguments for the superiority of AP over SVP. The chief exception to the usual pattern of comparison is Brams and Fishburn's work on four systems, which they call "ordinary approval" (our AP), "ordinary plurality" (our SVP), "runoff approval," and "runoff plurality" (Brams and Fishburn 1983, 40–71; Fishburn and Brams 1981). Their findings should have raised a caution flag against combining the approval ballot with nonplurality procedures generally. Yet, perhaps because of the prevailing linguistic ambiguity, advocates of approval balloting have recommended it in settings where decision rules other than the plurality principle are used.

Should the Brams-Fishburn warning against the approval runoff system be extended to other two-stage voting schemes? In order to address this question, we need a more precise typology of electoral decision rules.

## Types of Decision Rules

To avoid complexity, we concentrate our attention on one- and two-stage decision procedures. Processes involving three or more stages are not uncommon, but we foresee no serious problem in generalizing our results to them. We consider only those processes that culminate in the choice of a single winner, a simplification less severe than it may appear. Electing a parliament, for example, can be viewed as the first stage in a two-step process that ends with the victory of a single prime minister or government.

### One-Stage Rules

In a one-stage process, voters (using a balloting method) express their preferences only once. The best-known one-stage decision procedure is, of course, plurality rule, which is often favored precisely because it avoids the need for a runoff or other second-stage process. Plurality rule appears to be the only sensible rule that will ensure a first-stage choice with both SVB and AB. More complex ballots (e.g., the rank-order preference profile) can guarantee winners in one stage through more-complicated rules, such as the Borda count or transferable-vote algorithms.

### Two-Stage Procedures

In two-stage procedures, the initial expression of preferences has some effect on a second stage that ultimately determines the winner. The second stage may be a new vote by the original electorate, a vote by a different group of electors, a bargaining process involving several actors, or the unilateral decision of a single person. The first-stage vote may affect the second stage by narrowing down the field of candidates, by affecting who gets to decide at the second stage, or in both ways.

Two-stage procedures can become quite complicated, but many of them involve one or more of the following elements.

*Fixed Slate-Size Rules (FS).* Out of the $M$ original candidates, the $K$ who receive the most votes in the first stage form the slate that advances to the second stage ($1 < K < M$). For instance, search committees are commonly charged with proposing a fixed number of candidates to a decision maker.

*Threshold Rules.* Thresholds may be *sufficient* or *necessary*. A candidate who attains a *sufficient* threshold at the first stage wins without having to undergo a second test. Majority-or-runoff rules (with SVB) are the leading example of a sufficient threshold. Under a *necessary* threshold rule, any candidate who wants to advance to the second stage must reach the threshold in the first stage. A recent instance in the United States—made controversial by Jesse Jackson's challenge of it—was the 1984 Democratic party rule stipulating that presidential candidates who received fewer than 20% of the votes in a primary or caucus within a basic electoral unit were ineligible to receive any delegates selected by proportional representation to represent that unit.

The introduction of approval balloting necessitates a distinction between two versions of any threshold rule:

1. *Ballot Threshold Rule (BT).* A candidate reaches the threshold who receives votes on a proportion $T_b$ of the *ballots* cast in the first stage.
2. *Vote Threshold Rule (VT).* A candidate reaches the threshold who receives a number of votes equal to a proportion $T_v$ of the number of *votes* cast in the first stage.

Under single-vote balloting, there is no distinction between the BT and VT rules unless abstentions are recorded.[7]

*Proportional Representation (PR).* Under PR rules, the preferences expressed by voters at the first stage determine the composition of a smaller assembly that in some sense faithfully reproduces the sentiments of the initial electorate. All PR systems entail implicit thresholds at the first stage—the fewer the representatives per district, the higher the threshold; and some impose stricter explicit thresholds as well. In addition, second-stage assemblies commonly employ threshold rules (usually a majority requirement) in their own voting. PR systems are so important in democratic practice around the world that the possibility of combining them with approval balloting deserves extended consideration, which we reserve for a later paper. In the remainder of this paper, we focus on the interaction of AB with fixed-slate and threshold rules.

## Analysis and Results

Like original sin, strategic voting is a consequence of eating from the tree of knowledge. Under AB, a rational voter who has no idea how anyone else plans to vote will follow a purely sincere strategy. Similarly, under SVB, an ignorant voter maximizes expected utility by sincerely selecting his or her most-preferred candidate. Voters who maximize expected utility will vote strategically—that is, depart from pure sincerity—only when they have information about others' probable voting strategies. Such knowledge comes from two sources, which we call *collusion* and *polling*. *Collusion* is an agreement among a subset (or coalition) of voters to coordinate their voting strategies. *Polling* is shorthand for any other source of advance information about preferences or probable choices of other voters.

Although real-life collusion may bring into play such considerations as keeping one's word, social pressure, or coercion, we assume only that collusion affects the strategy of a rational, uncoerced voter in the same way as does polling—by altering the voter's estimates prior to voting of the level of support one or more candidates will receive. A voter invited to join a coalition with $S$ others can predict (to the extent the voter believes the coalition will stick together) that candidates the coalition agrees to support will receive $S$ votes more than whatever baseline levels the voter expects they will receive from the $N - S - 1$ voters who are not part of the conspiracy. Similarly, the voter knows that candidates the coalition agrees not to support will not exceed their baseline levels.

Most of the analyses that follow focus on voting sincerity under approval balloting, but we also make comparisons with single-vote balloting when conclusions about that method are already available or readily apparent. Although our results depend on the decision rule with which approval balloting interacts, we find a common framework of analysis useful for investigating strategic voting under all the rules that we consider. We assume throughout a utility-maximizing focal voter who assigns utilities $u_1, \ldots, u_M$ to the $M$ candidates.

## One-Stage Systems

For the plurality decision rule, let $s_j$ be the focal voter's estimate of the relative probability that candidate $j$ will be in a tie for first place. (By *relative probability* we mean that the $s_j$'s are normalized so that $\Sigma\, s_j = 1$.)[8] Given that there is a tie for first place (in which the tied candidates are unknown), the expected utility to the voter of a tied candidate is

$$\sum_j s_j u_j .$$

Hence, under AP, the voter has incentive to cast a tie-breaking approval vote for candidate $i$ if

$$u_i > \Sigma\, s_j u_j .^{9} \qquad (1)$$

Note that such a strategy involves no skipping.

Previous research indicates that approval plurality consistently dominates single-vote plurality with respect to sincerity (Brams and Fishburn 1983, 1985; Merrill 1982). Under AP with trichotomous preferences and given prior information from polling or collusion, the voter may depart from pure sincerity but only toward expansive or restrictive sincerity.[10] For the general case of multichotomous preferences, Brams and Fishburn's (1983) Corollary 2.1 implies that no admissible AP strategies are strongly insincere, but they also report (p. 30) that some admissible AP strategies involve skipping. However, the only such cases that we can imagine require improbable circumstances.

In short, whereas strategic voting under single-vote plurality entails frequent and obvious strong insincerity, we can state the following conclusions about approval plurality:

AP 1. Under approval plurality, rational voters never have incentive to cast strongly insincere ballots.

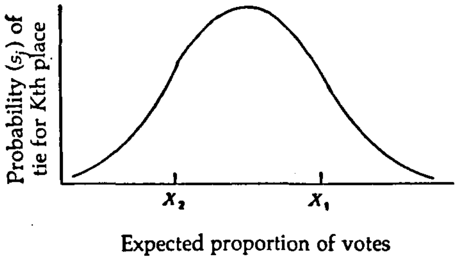
AP 2. Under approval plurality, rational voters rarely have incentive to cast weakly insincere ballots.

AP 3. Under approval plurality, strategic voting will normally be limited to expansive and restrictive sincerity.

### Two-Stage Systems

To examine the interaction of approval balloting with two-stage procedures, let us consider first the fixed-slate-size (AB-FS) and ballot-threshold (AB-BT) rules. For rule FS and slate size equal to $K$, let $s_j$ be the focal voter's estimate of the relative probability that candidate $j$ will be in a tie for $K$th place before the voter votes, since attaining $K$th place (or better) is tantamount to attaining the slate. If we assume that every candidate who gets

**Figure 2. Probability of a Decisive Vote versus Expected Proportion of Votes**



onto the slate has an equal chance of winning at the second stage, then, if there is a tie for *K*th place, the expected utility to the voter of an unknown tied candidate is $\Sigma\, s_j\, u_j$. Hence, under FS, the voter has an incentive to cast a tie-breaking approval vote in favor of candidate *i* if

$$u_i > \Sigma\, s_j\, u_j. \qquad (2)$$

In order to compare this condition with a comparable criterion for the BT rule, let $q_j$ be the voter's estimate of the relative probability that candidate *j* will attain or exceed threshold $T_b$. Here the expected utility of the slate that survives to the second stage is $\Sigma\, q_j\, u_j$. Again assuming no information about the outcome in the second stage, this quantity is the expected utility to the focal voter of the slate. Voting for (or against) a candidate may permit the voter to add the candidate to (or delete him or her from) the slate, but not to *replace* a candidate as under the FS rule. Thus under BT the voter has incentive to cast an approval vote for candidate *i* that enables the candidate to reach $T_b$ if the voter prefers the candidate to the expected utility of the slate, that is, if

$$u_i > \Sigma\, q_j\, u_j. \qquad (3)$$

Comparison of strategies for FS and BT thus reduces to an analysis of the probabilities $s_j$ and $q_j$. Because $1 < K < M$, the *K*th position in the vote totals is an intermediate one. Therefore, $s_j$ will be highest for candidates of intermediate strength—in other words, the front-runner or a sure loser is less likely to be in a tie for the *K*th place. Thus, a plot of $s_j$ versus expected proportion of the vote should look like Figure 2.

Accordingly, in the FS case, $\Sigma\, s_j\, u_j$ will tend to be close in value to the utilities of candidates near the *K*th position. For example, suppose that there are five candidates $c_1, \ldots, c_5$, ranked in descending order of expected electoral strength and that $K = 3$. Then the middle candidates are more likely to tie for third place so that assignments of, say, $s_1 = .1$, $s_2 = .2$, $s_3 = .3$, $s_4 = .3$, and $s_5 = .1$ are plausible. The expected utility ($.1u_1 + .2u_2 + .3u_3 + .3u_4 + 1u_5$) weights $u_3$ and $u_4$ more heavily, so it is more likely to be near the value of $u_3$ or $u_4$; but these trailing candidates have low average utility for most voters (that is why they are trailing). So $\Sigma\, s_j\, u_j$ tends to be low for most voters. Combined with Criterion 2, this conclusion leads to two results, which should be interpreted in the sense of expected values over votes and elections:

FS 1. Under fixed-slate-size rules, most voters have an incentive to cast approval votes for more candidates than under approval plurality.
FS 2. This effect will be more pronounced when *K* is large relative to *M*.

In contrast, under AB-BT, the probability of attaining the threshold will increase monotonically as candidate strength increases, yielding a plot like Figure 3. This characteristic of $q_j$ implies that the expected mean utility of the slate will be more strongly affected by the utilities of the front runners, which are of course high for most voters. This is especially so if $T_b$ is high. Therefore, Criterion 3 leads to two results:

BT 1. Under ballot threshold rules, most voters have an incentive to cast fewer approval votes than under AP or AB-FS rules.
BT 2. The tendency to truncate voting strategies will be stronger, the higher the threshold.

To investigate strategy under AB-VT, we assume that, at most, one candidate is near enough to $T_v$ that the candidate's prospects of attaining it can be affected by the focal voter. Suppose the focal voter wishes to know whether to vote for candidate *i*. If *i* is the candidate near $T_v$, then Criterion 2 applies: vote for *i* if $u_i > \Sigma\, q_j\, u_j$. Because this is the same as for the ballot threshold situation, we obtain two similar results:

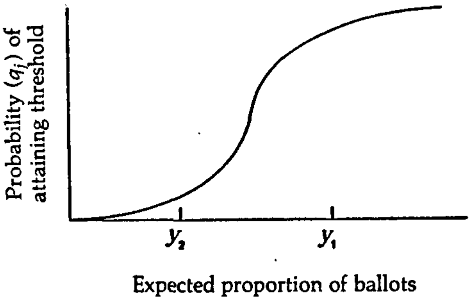
VT 1. Under vote threshold rules, most voters have an incentive to cast fewer approval votes than under AP or AB-FS rules.
VT 2. The tendency to truncate voting strategies will be stronger, the higher the threshold.

However, strategies under VT are not a carbon copy of those under BT. Suppose some other candidate *k* is near $T_v$. Then a vote for *i* has the potential of dropping *k* from the slate because every additional vote increases the denominator on which $T_v$ is calculated and so *dilutes* the vote proportions of all other candidates (including *k* who is near the threshold). The voter should cast this sort of indirectly negative vote if

$$u_k < \Sigma\, q_j u_j. \tag{4}$$

Because $u_k$ is the utility of a candidate who marginally makes the slate, whereas $\Sigma\, q_j\, u_j$ is an average of utilities of all candidates who attain the slate, the latter is typically higher than the former. Hence, in most cases, Criterion 4 is true, so that the voter may be rational to vote for *i* even though $u_i < \Sigma\, q_j u_j$. Thus Criterion 4 leads to the following result:

**Figure 3. Probability of a Decisive Vote versus Expected Proportion of Ballots**



VT 3. Under vote threshold rules, most voters have an incentive to cast more approval votes than under ballot threshold rules.

The incentive to dilute under VT will normally generate expansively sincere strategies, but under more stringent assumptions (to be described later), skipping is also possible.

As a corollary to this argument, we note that VT—unlike BT—is sensitive to the number of candidates. Under VT a front-runner might encourage additional candidacies in order to pull rivals below $T_V$.

*The Effect of Collusion.* We now proceed one step further and replace the focal voter by a coalition (with common utility profiles) large enough to alter significantly the probability that candidates make the slate. Under FS, a decision by the coalition to withhold votes from a candidate *i* otherwise expected to make the slate may well remove the candidate from the slate. Such an agreement will typically move *i*'s proportion of the vote from, say position $x_1$ in Figure 2 to position $x_2$, that is, from better than *K*th place to worse than *K*th place. However, because the graph is unimodal, the value of $s_i$ may increase or decrease but is not likely to change greatly (remember that $s_i$ is the probability that $i$ will *tie* for $K$th place). In turn, Criterion 2 will probably be little affected.

In contrast, under BT, the coalition's withholding of votes from candidate $i$ may reduce the candidate's expected proportion of the ballots from $y_1$ to $y_2$ (see Figure 3), but here the shift greatly decreases the value of $q_i$. Hence $\Sigma\ q_j\ u_j$ becomes a weighted average of the utilities of candidates who remain on the slate —typically a higher value that reduces still further the number of candidates who should be supported. This reverse snowball effect implies that it may be rational for a coalition to vote against a candidate who initially satisfies Criterion 3 but who will fail to meet that condition after the coalition has withheld votes from some *other* candidate.

We thus have additional results about the effects of collusion under the FS and BT rules:

FS 3. Under AB-FS rules, collusion creates only modest inducement for coalition members, as compared to noncolluders, to truncate or plunk.

BT 3. Under AB-BT rules, collusion creates a strong inducement for coalition members to truncate and, often, to plunk.

*Simulations.* To substantiate the preceding analytic results, we conducted a number of computer simulations. Each basic simulation run involves five hundred elections, each with five candidates and one hundred voters chosen from a standard bivariate normal spatial model (correlation = 0) (a few runs with five hundred voters give very similar results). All runs (unless otherwise noted) assume the Shepsle (1972) utility function $u(d) = e^{-d^2/2}$, where $d$ is the euclidean distance between voter and candidate.[11]

Under AB-FS, with slate of size $K$, a voter votes for a candidate whose utility exceeds $\Sigma\ s_j\ u_j$, where $s_j$ is the relative probability that candidate $j$ will tie for $K$th place. Assume the candidates are ranked in descending order by a poll (i.e., candidate 1 is expected to finish first, etc.).

For the limiting case where $K = 1$ (i.e., approval plurality), we used four different assumptions to assign values to the $s_j$:

1. $s_j = x_j$, where $x_j$ is the proportion of the vote that $j$ receives in a simulated election assuming pure sincerity
2. $s_j = x_j^2$
3. The Brams poll assumption (Brams and Fishburn 1983, chap. 7), in which a voter retains his or her prepoll strategy if he or she voted for exactly one front runner; otherwise the voter casts approval votes for all candidates whose utilities equal or exceed $\max(u_1, u_2)$
4. The $s_j$ are proportional to (5,4,3,2,1)

To perform simulations with slates of size $K > 1$, for which Assignments (1) and (2) are not meaningful, we used the following procedure: for $K = 2$, the $s_j$ are proportional to (3, 4, 3, 2, 1); for $K = 3$, the $s_j$ are proportional to (1, 2, 3, 2, 1); and for $K = 4$, the $s_j$ are proportional to (1, 2, 3, 4, 3). To simulate the AB-BT system, we assumed that the weights $q_j$ are proportional to a linear approximation (with slope $c$) of the graph in Figure 3.

Results from these simulations, which are presented in Table 1, confirm all our previous conclusions. Before the introduction of polling information, the proportion of voters plunking (under the Shepsle utility assumptions) is constant at about 15% (subject only to small random variation). As predicted by BT Results 1 and 2, polling information induces voters to restrict their strategies in the presence of a ballot threshold, so much so that plunking increases to 35% and 43%

(depending on the value of $c$) with a majority-rule threshold. It is noteworthy, however, that a low threshold of 20% has a relatively slight restrictive influence.

Consistent with FS Result 1, strategies under fixed-slate-size rules are generally less restrictive than under BT. FS Result 2 is confirmed by the tendency for plunking to decrease as $K$ rises. Indeed, for $K = 4$, polling information induces slightly more expansive strategies. In the limiting case of $K = 1$ (approval plurality), plunking increases substantially in some cases. The Brams poll assumption requires plunking if the voter, before the poll, voted for both front-runners identified by the poll and one of them is the voter's first preference. In our simulated contest, the proportion who plunk is 43%.

In Table 1 we also present simulation results for alternative utility functions and for variations in the number of candidates, assuming, in each case, the plurality decision rule (or equivalently, the FS rule with $K = 1$). Although both factors strongly affect the percentage who plunk, they do not change our conclusion about the effects of polling, which appears to exert a relatively independent influence.

We also simulated the effect of collu-

**Table 1. Likelihood of Optimal Plunking under Approval Voting without Collusion (Simulation Results)**

| | | Voters Plunking (%) | | Mean No. of Candidates Voted for | |
|---|---|---|---|---|---|
| Independent Variables | Number of Candidates | Before Poll | After Poll | Before Poll | After Poll |
| Approval plurality ($K$=1)[a] | | | | | |
| $s_j = x_j$ | 5 | 15 | 24 | 2.18 | 1.96 |
| $s_j = x_j^2$ | 5 | 15 | 32 | 2.18 | 1.83 |
| Brams rule | 5 | 15 | 43 | 2.18 | 1.80 |
| Fixed-Slate-Size rule[a] | | | | | |
| $K = 2$ | 5 | 15 | 21 | 2.17 | 2.00 |
| $K = 3$ | 5 | 15 | 16 | 2.18 | 2.17 |
| $K = 4$ | 5 | 14 | 12 | 2.19 | 2.35 |
| Ballot-Threshold rule[a] | | | | | |
| $T_b = .50$ ($c = 2$) | 5 | 15 | 35 | 2.19 | 1.82 |
| $T_b = .50$ ($c = 10$) | 5 | 15 | 43 | 2.18 | 1.72 |
| $T_b = .20$ ($c = 2$) | 5 | 15 | 20 | 2.18 | 2.04 |
| Alternative utility functions ($K = 1$)[b] | | | | | |
| $u(d) = \exp(-d^2/1)$ | 5 | 31 | 40 | 1.85 | 1.71 |
| $u(d) = \exp(-d^2/2)$ | 5 | 15 | 24 | 2.18 | 1.96 |
| $u(d) = \exp(-d^2/4)$ | 5 | 6 | 13 | 2.45 | 2.17 |
| $u(d) = -d$ | 5 | 2 | 10 | 2.60 | 2.25 |
| Variation of number of candidates ($K = 1$)[b] | | | | | |
| 3 candidates | 3 | 64 | 73 | 1.36 | 1.27 |
| 4 candidates | 4 | 34 | 48 | 1.74 | 1.56 |
| 5 candidates | 5 | 15 | 24 | 2.18 | 1.96 |
| 8 candidates | 8 | 2 | 2 | 3.42 | 3.05 |

[a] Each simulation is based on 500 elections, each involving 100 voters (standard errors for plunking percentage are about 1 percentage point).

[b] Each simulation is based on 100 elections, giving standard errors of 2 to 3 percentage points.

Table 2. Likelihood of Optimal Plunking in the Presence of a Colluding Coalition (Simulation Results)

| Independent Variables | Number of Candidates | Oceanic Voters Plunking (%) Before Poll | Oceanic Voters Plunking (%) After Poll | Colluding Coalitions Plunking (%) |
|---|---|---|---|---|
| Fixed-Slate-Size rule ($K = 1$)[a] | | | | |
| 10% coalition | 5 | 16 | 23 | 31 |
| 20% coalition | 5 | 15 | 24 | 40 |
| Ballot-Threshold rule ($T = .5$; $c = 2$)[a] | | | | |
| 10% coalition | 5 | 16 | 37 | 49 |
| 20% coalition | 5 | 16 | 38 | 64 |

[a]Each simulation is based on 100 elections, each involving 100 voters; standard errors for oceanic voters: 2 to 3 percentage points; for colluding coalitions: 4 percentage points.

sion among coalitions representing 10% and 20% of the voters for approval plurality and for a majority BT rule. We imagine the coalition operating in an ocean of voters acting independently. We therefore assume that while the oceanic voters vote in ignorance, the colluding voters use the strategy of their coalition—that is, knowledge of their approved set—as the equivalent of polling information. The capacity of a coalition to move a candidate across a threshold depends on its own size, the height of the threshold, and the level of support the candidate receives in the ocean. Although we assume the coalition is ignorant of that oceanic support, it is not realistic to assume that the oceanic support for each candidate is the same. Instead, we poll the oceanic voters to determine candidate support; but the coalition is assumed to know only the *distribution* of candidate support, not the identity of candidates having each level of support. Each of these simulation runs involved one hundred elections, each consisting of one hundred voters, of whom 10% and 20% formed colluding coalitions.

Collusion increases plunking substantially in the AP case, but its effect is dramatically stronger with a majority BT. In Table 2 we also show that the encouragement of plunking increases with the size of the coalition, for both AP and AB-BT voting systems. Under the majority threshold (and setting $c = 2$), a 20% coalition has a 64% probability that it should plunk. This level of plunking compares with 15% plunking among nonstrategic voters and 38% plunking among oceanic voters when they have polling information but no knowledge of the conspiracy.

## Insincerity

So far, our analysis has dealt with the most obvious and (we would expect) most probable form of AB strategic voting—restrictive sincerity and its limiting case, (sincere) plunking. If one deems an incentive to truncate undesirable, AB-BT systems emerge from this analysis less favorably than AB-FS and AB-VT systems. However, another strategic voting possibility under AB may cut in favor of BT compared with FS and VT. This is the strategy of skipping in order to set up a *pushover* opponent for one's favorite in the second stage. To see how this works, imagine a three-candidate AB-FS race with $K = 2$. Let the focal voter prefer $c_1$ to $c_2$ and $c_2$ to $c_3$. Suppose the voter (1) is confident that his or her favorite $c_1$ can make the slate, (2) believes the favorite will win if paired against $c_3$ in the second stage but (3) is not so sure the favorite can beat $c_2$. Then the best strategy is to approve $c_1$ *and* $c_3$. This behavior is weakly insincere because the voter has skipped a second choice, $c_2$.[12]

AB-BT systems offer less incentive to back a less-preferred pushover because helping the pushover reach the threshold does nothing to eliminate any other candidate; in the example above, skipping to $c_3$ might merely enable all three candidates to reach the second stage.[13] AB-VT systems create an intermediate incentive to approve a pushover. By casting an extra approval vote for $c_3$, the voter dilutes $c_2$'s proportion of the vote. In a small-group situation or for a relatively large coalition, skipping might therefore depress the vote proportion of the real threat enough to make boosting a pushover worthwhile.

Brams and Fishburn (1983, chap. 3) point out the possibility of the pushover strategy in connection with AB runoff systems, which might at first glance seem to contradict our argument that a ballot threshold does not create this incentive for weak insincerity. However, the usual runoff rule is actually a hybrid. Under SVB, it consists of a sufficient ballot threshold of .5. If no one reaches that standard, the runoff becomes an FS rule with $K = 2$. Although there are several conceivable runoff rules under AB, in each case the possibility of a pushover strategy arises from the FS aspect of the runoff hybrid, not from its use of a ballot threshold.

How serious is the risk of skipping to create a pushover? As noted above, in both the FS and VT cases, the strategy depends on the conjunction of three beliefs, two of which—(1) and (2)—must be held with great confidence; otherwise the voter (or coalition) sets up a chance for the worst possible outcome. In addition, any tendency, on the part of the agent making the second-stage choice, to look favorably on a high initial vote total should serve as a brake on the voter's incentive to approve a nonpreferred candidate. In short, insincere voting to create a pushover seems a real but unlikely possibility under AB; it would be most probable when decision rules have FS elements, considerably less probable under VT, and quite improbable when BT is unalloyed by FS features.

## Discussion

Under approval plurality, strategic voting will occur, but we expect it will be relatively benign. Voters need never resort to decapitation and seldom to expansive sincerity and skipping. Strategic behavior under AP will be confined largely to truncation, including plunking. However, our belief that utility functions are negatively skewed implies that even nonstrategic voters will approve a smaller proportion of candidates than other writers have expected they would. Nevertheless, polling and collusion will increase the tendency to plunk, sometimes sharply. Therefore, we need more analysis of the implications of such strategies, especially with respect to equity among voters.

Compared with approval plurality, two-stage AB systems generally impose on the voter more difficult strategic choices and are thus likely to produce more-complex strategic behavior. The most frequent tendency under two-stage systems, as under AP, will be toward restrictive sincerity, including plunking. Voters who coordinate their strategies are especially likely to truncate. Ballot-threshold rules are more conducive to truncation than are fixed-slate-size rules. Vote-threshold rules seem intermediate between these two in their encouragement of truncation, but VT rules also may lead to other forms of strategic behavior—

expansive sincerity and the encouragement of additional candidacies in order to dilute opponents' proportions of the vote. In addition, VT and FS rules both can promote a more advanced kind of strategic voting—skipping motivated by desire to set up a pushover opponent for one's favorite. However, this behavior seems to us unlikely in large electorates.

These difficulties with multistage AB systems do *not* imply that such voting schemes will be less desirable than their SVB counterparts. Indeed, they may very well prove superior. The comparative analysis needed to justify a claim either way would assess strategy proofness, Condorcet efficiency, and equity in distributing power among voters. We can recommend, however, that whenever one is able to choose between approval plurality and multistage AB systems, the former is definitely preferable with respect to the goal of avoiding strategic voting. In other words, it is best to avoid complicating approval balloting with thresholds, runoffs, and similar encumbrances.[14]

There are many situations, however, where second stages are essential for other reasons or where they are already irremovably institutionalized. In some of these (e.g., search committees), a preliminary winnowing of wheat from chaff must be accomplished. When AB is used at the first stage, should this narrowing be accomplished by thresholds or by fixed-slate-size rules? In most situations, we recommend FS over either BT or VT. Because it is riskier and more blatant, the skipping to which FS is vulnerable seems to us less probable than the truncation that thresholds so strongly encourage.[15]

What about two-stage processes (e.g., parliaments and nominating conventions) in which preliminary narrowing of the field is less essential? If one's only goal is to minimize strategic voting under AB, then in such cases it would be best not to impose thresholds or fixed-slate-size rules. However, because our simulations indicate that thresholds as high as 20% may only slightly increase the rate of plunking, we would not argue against the use of approval balloting with low thresholds.

Approval balloting is an exciting subject primarily because of its potential as a practical reform. It may be of interest therefore to conclude with a summary of our views as to where the approval ballot should and should not be employed, given the present state of knowledge about the method.

When the existing alternative is single-vote plurality, we would recommend approval plurality as a replacement in elections not contested by parties or wherever the two-party system has hopelessly broken down. Appropriate situations include nonpartisan local elections and party primaries that choose candidates rather than convention delegates.[16] We are reluctant to recommend AP in partisan general elections only because we fear its possible effect over time on the two-party system (cf. Riker 1982, 113).

For multistage decision processes—including parliamentary elections and the choice of delegates to U.S. presidential nominating conventions—we urge for the time being that advocates of approval balloting be zealous scholars but cautious reformers. The research directions identified in this paper need to be followed farther before we can endorse approval balloting for multistage processes that shape the fate of nations. However, on a smaller scale and in less momentous situations, we urge experimentation whenever possible—not simply because we need data to test theories but also because the behavior of real voters is the best stimulus to extending and improving theory. Consequently, we would support the use of approval balloting in one or more state presidential primaries even though we are not presently ready to recommend that national parties push for its use in all states.

## Notes

The authors would like to thank Steve Brams, Gary Cox, and Paul Quirk for helpful suggestions. This research was partially supported by a grant from the UPS Public Policy Initiatives Fund of the University of Pennsylvania.

1. We explain later why we prefer *approval ballot* or *approval balloting* to the usual *approval voting*.

2. Etymologists may be able to link the word to the British term *plumping* (Cox 1981).

3. We can think of three reasons why one might approve candidates of below-average utility: (1) if the possible outcomes include failure to select any winner and if that prospect is sufficiently horrible, a sensible voter might choose to approve every candidate (this case can be collapsed into pure sincerity by treating "no one" as an implicit candidate whose relative utility is so low that all real candidates become, like the children of Lake Wobegon, above average); (2) if both front-runners (as revealed by polls) have below-average utility for a particular voter, the voter might reluctantly decide to approve the one he or she considers less bad along with all candidates he or she ranks higher than the lesser of the two evils; (3) as we show later, a strategic voter will sometimes vote for a below-average candidate in order to manipulate the outcome of a two-stage process.

4. The relation of approval balloting to multiple-victor elections ($K > 1$) deserves further investigation. For a possible starting point, see Cox 1981.

5. Brams and Fishburn (1983, 22–23) restrict the voter to $M - 1$ votes, which makes sense with a plurality decision rule. However, as we noted earlier, in some two-stage processes, a voter may approve all candidates without being fatuous.

6. For the general case where $K > 1$ and the voter can cast one vote per office, we propose the term *selection ballot*. Following Joslyn (1976), one of us previously called this type of ballot *categorical voting* (Merrill 1981). We now consider *selection ballot* preferable both because it provides a verb (*select*) to parallel *approve* and because the dictionary definition of *select* as "chosen in preference to another or others" seems especially appropriate in this context.

7. If AB were employed in large-scale elections, a BT rule would require changes in present counting procedures. Except when there is only one contest on the ballot, it would become necessary to keep track of the number of people who vote for each office. This complication could be avoided by defining the ballot threshold in terms of the number of citizens who turn out, rather than the number who vote for the particular office in question.

8. If the probabilities of all ties are zero, the voter has no chance of affecting the outcome, and the voter's strategy is irrelevant.

9. A seemingly more straightforward approach to the voter's decision would have the voter compare the expected utilities of various strategies—for example, voting only for candidate 1 vs. voting for candidate 1 and candidate 2. Under the following two plausible assumptions, this line of attack also ends up at Criterion 1. Assume (1) that the number of voters is large enough that the probability that three or more candidates are within two votes of one another is neligible relative to the probability of a tie between two of them and (2) that the probability that candidate $i$ is tied for first place with candidate $j$ is proportional to the product of the relative probabilities $s_i$ and $s_j$. Interested readers may obtain details from the authors.

10. Result 12 in Niemi 1984 demonstrates both possibilities.

11. Because our results are sensitive to assumptions about the voter's utility function, we justify our choice of the Shepsle function in a separate appendix that is available upon request from the authors.

12. The pushover strategy can also produce expansively sincere voting (e.g., if $c_2$ were the pushover). Cf. n. 3 above.

13. Whether there would be any other reason for insincerity depends on the choice mechanism at the second stage.

14. Quite aside from worries about strategic voting, majority thresholds are less needed with AB because it is so much more likely than SVB to produce majority support for the leading candidate.

15. Strategic voting might also be more prevalent under threshold systems because the necessary calculations are easier to make. Estimating whether a candidate will attain a vote total near a threshold involves only one unknown. In contrast, under an FS rule, one must estimate whether a candidate will tie for $K$th place, which involves several unknowns.

16. It may be objected that primaries are always the first stage in a two-stage process. We believe, however, that the prospect of a general election to follow usually will not affect the strategies of primary voters. The principal exception would be crossover voters in open primaries—but insincerity on their part is just as much a danger with SVB as it would be with AB.

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# THE EFFECT OF ELECTORAL REWARDS IN MULTIPARTY COMPETITION WITH ENTRY

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*The authors elaborate a model of electoral competition for a fixed number of seats in a legislature. The novel feature of this model is that candidates (or parties) not only choose spatial locations as platforms but also determine whether to enter the contest at all. In most previous spatial models, the set of candidates is specified exogenously. Here, however, the spatial positions* and *the set of candidates are determined endogenously. An equilibrium in this context is defined and results are proved, suggesting that entry may disrupt spatial equilibria. Finally, the authors compare their treatment of spatial competition with entry to that of Palfrey.*

Electoral systems possess numerous features on which they may be compared and contrasted. The main feature of such systems, however, is the method by which votes cast by citizens are aggregated and winners determined. How this transformation of citizen votes into elected officials is effected has manifold consequences. Two of the most important strategic aspects are how existing candidates compete against each other for votes and whether a potential candidate decides to activate his or her candidacy. That is, the manner in which votes are aggregated and officials elected conditions not only the strategic interaction among the players of the electoral game but also determines, in large part, who those players will be by encouraging or deterring entry.

For purposes of concreteness, let us consider two broad classes of electoral system. The first, which we call the *fixed-standard method,* sets down a standard, or criterion. Those candidates who surpass it are deemed elected, and those who fail to achieve it are defeated. A specific example is the uniform-quota method of Reichstag elections in Weimar, Germany. According to Hoag and Hallett (1926, 412), "an arbitrary quota is fixed for all districts. In German Reichstag elections it is fixed permanently at 60,000. In each district, each party is assigned as many seats as there are quotas in its vote." Under this arrangement, a standard is provided exogenously, and party seats, district delegation size, and even the size of the legislature are determined endogenously.

The second class of electoral system, operating according to the *fixed-number method,* is much more common. According to this method, each electoral district is allotted a fixed number of seats ($K$), and parties or candidates are awarded these seats on the basis of *relative* performance at the polls. Of course, $K = 1$ describes the familiar "first-past-the-post" plurality method common in Anglo-American elections. In contrast to the fixed-standard method, here the number of elected officials is fixed exogenously, and the standard required for election is determined endogenously.

Each of these classes of electoral system induces distinct candidate objectives, behaviors, and prospects for equilibrium. By the latter we mean a circumstance in which each announced candidate plays an electoral strategy and has no incentive to alter it, while no other potential candidate has an incentive to enter the contest. A recent paper by Greenberg and Weber (1985) took up precisely this issue for fixed-standard electoral systems. In a multiparty context, they provided a fairly standard setting—finite set of candidate positions; finite number of voters; complete, transitive, single-peaked voter preferences; and fixed standard, or quota, of $m$ votes for election—in which there is an equilibrium. There is a set of (running) candidates each choosing an alternative as its platform, each receiving at least $m$ votes, and therefore each elected, while no other (potential) candidate chooses to run, being unable to secure the necessary $m$ votes for any available platform.

In the present paper, we extend this analysis to the more common fixed-number electoral method. A precise definition of equilibrium (with potential entry) is given in the next section. We then prove our main result, namely, that when the size of the legislature is predetermined, an entry-deterring multiparty equilibrium may fail to exist, even under rather strong assumptions—assumptions, in particular, that guarantee the existence of an equilibrium under the uniform-quota method. We conclude the paper with a discussion of our equilibrium concept and a comparison of it to a related concept due to Palfrey (1984).

Our analysis, like that of Greenberg and Weber (1985) and that of Palfrey (1984), emphasizes the importance of potential entry on the existence of equilibrium and on the properties of equilibrium behavior. We note that most previous studies of electoral equilibrium take the set of candidates as given. In Downs's (1957) famous median-voter theorem, for example, an answer was provided to the question of which strategies would be played in equilibrium by two *existing* electoral competitors. In contrast, the basic feature of the equilibrium notion we propose permits (but, in equilibrium, deters) potential entry. In our model, the set of actual candidates is determined as part of the equilibrium. As a result, it is impossible in equilibrium for nonactive candidates to choose electoral positions that would guarantee them seats. The mere prospect of entry, however, may discourage competitors from converging toward each other (as in the Downsian model) and, in fact, may destroy equilibrium altogether.

Although we are chiefly engaged in a piece of comparative electoral analysis, it should be understood that the ideas we offer extend beyond the electoral arena. In considering the methods by which societies generally distribute prizes, privileges, or rewards, one observes fixed-standard and fixed-number methods practiced. The fixed-standard method, allowing the number of prize winners to vary with performance, is common in many professional-licensing contexts. Some form of examination is administered, and all who exceed a predetermined test score are said to have "passed" and are granted licenses. On the other hand, the fixed-number method, in which the number of prize winners is set in advance, is a common feature in incentive contracts. For example, the performances of sales personnel in an organization or firm are rank-ordered (say, by dollar volume), and the $K$ best performances are awarded

Table 1. Relationship between Electoral System and Candidate Objectives

| System | Candidate Objective<br>Maximize Votes (Up to Quota) | <br>Maximize Rank (Up to $K$) |
|---|---|---|
| Fixed-standard | appropriate | questionable |
| Fixed-number | questionable | appropriate |

"bonuses." Each method is characterized by two parameters—number of prizes and level of performance—but they differ as to which is fixed exogenously and which is determined endogenously.

As a final introductory point, we suggest that the method of awarding prizes affects not only electoral outcomes and candidate behavior but, at a very fundamental level, candidate objectives as well. If candidates choose strategies ($x_i$ for candidate $i$), then, in general,

$$u_i^{FS}(x_1, x_2, \ldots, x_n) \neq u_i^{FN}(x_1, x_2, \ldots, x_n)$$

where the superscripts stand for "fixed standard" and "fixed number," respectively and $u_i$ is $i$'s utility function. Candidate $i$ evaluates the outcome produced by electoral strategies $x_1, x_2, \ldots, x_n$ *differently* depending on the system. In effect, $i$ employs a utility function that is system-dependent. This implies that the nature of maximizing behavior is system-dependent as well.

This would seem to require that candidate-objective functions be formulated with the particular system being modeled in mind (see Table 1). We maintain that a fixed-standard (quota) system encourages vote maximization (possibly of a satisficing sort, with the quota serving as a "level of aspiration"). Finishing among the top $K$ performers may be of no help to a candidate if that candidate fails to exceed the quota; and finishing below the top $K$ may cause no harm so long as he or she does exceed the quota. Likewise, a fixed-number system encourages rank maximization (again, possibly of a satisficing sort, with position $K$ serving as the aspiration level). Vote maximization in this setting seems most inappropriate as a candidate objective because there are circumstances (see the "Limit-Equilibrium" section) in which vote maximizing may actually reduce a candidate's rank below $K$.

The point here is that the electoral system determines optimal behavior by making some objectives more productive than others. In our view, the off-diagonal cells of Table 1 are behavioral objectives inappropriate to the given method of determining winners. This has important consequences for modeling choices, which we take up after presenting our main result.

## The Model and Principal Result

Let $T$ denote the set of voters. Each $t \in T$ has a preference ordering, denoted $\geqslant_t$, over the set of alternatives, $\Omega$. We impose three very restrictive assumptions that, in view of the ("negative") nature of the principal result, imply that it is unlikely to find a plausible set of assumptions that will guarantee the existence of an equilibrium in other than idiosyncratic circumstances. The first assumption, concerning the topological structure of $\Omega$, is

ASSUMPTION 1. *$\Omega$ is a compact subset of the real line $R$.*

Hence, alternatives can be "ordered" (numbered). Note that Assumption 1 allows for both a finite and an infinite set of alternatives.

The second assumption states that voter preferences $\succcurlyeq_t$ can be represented by the distance of the alternatives to voter $t$'s "peak," or "ideal point," $q(t)$.

ASSUMPTION 2. *For each voter $t \in T$ there exists a unique most preferred alternative $q(t) \in \Omega$ such that for any two alternatives $a$, $b \in \Omega$, $a >_t b$ if and only if $|a - q(t)| < |b - q(t)|$.*

The final assumption is that individuals vote "naively," or "sincerely," and not in a "manipulative," or "strategic," way.

ASSUMPTION 3. *Given a set of proposed alternatives, each voter $t \in T$ votes for his or her most preferred alternative within that set.*

Because of Assumption 2, a voter $t \in T$ is fully characterized by his or her ideal point $q(t) \in \Omega$. Therefore, the society is completely specified by the set of alternatives $\Omega$ together with a distribution function $F$ of the ideal points of the voters. Specific assumptions about $F$ will be made explicit as they are invoked.

Let us now turn to the definition of equilibrium in the fixed-number electoral system. An exogenously given positive integer, $K$, represents the number of seats in the legislature (or a legislative district), that is, the number of candidates to be elected. A *K-equilibrium* is the set of $K$ candidates each proposing a (different) alternative in $\Omega$, so that no new (potential) candidate, by offering an additional alternative, can attract a sufficient number of voters to guarantee himself or herself a seat. To characterize a $K$-equilibrium formally, we need first to define the support received by a candidate offering the alternative $a^*\in \Omega$ when all other candidates offer alternatives from $A \subset \Omega$. By Assumption 3, this is simply the set of voters in $T$ with ideal points $q(t)$ closer to $a^*$ than to any alternative in $A$.

DEFINITION 1. *Let a society be given by $(\Omega, F)$ and let $A$ be a* finite *subset of alternatives in $\Omega$. The* support *of alternative $a^* \in \Omega$, given $A$, denoted $S(a^*; A)$, is the set of all voter ideals in $\Omega$ that are closer to alternative $a^*$ than to any other alternative in $A$. That is,*

$$S(a^*; A) = \{q(t) \in \Omega\colon |q(t) - a^*| < |q(t) - b| \ \forall\, b \in A \,/\, \{a^*\}\}$$

*The* measure *of the support of $a^*$, given $A$, denoted $s(a^*; A)$, is the relative size of $S(a^*; A)$, and is a given by*

$$s(a^*; A) \equiv \int_{S(a^*;\, A)} f(c)dc$$

*where $f$ is the density function (assumed to exist) that corresponds to the distribution function $F$ of the voter-ideal points.*

When $\Omega$ is finite, the measure of the support of an alternative $a^* \in \Omega$, given the (finite) set $A$, is equal to the number of voters who strictly prefer $a^*$ to any other alternative in $A$, that is,

$$s(a^*; A) = \#\{t \in T : |q(t) - a^*| < |q(t) - b| \ \forall\, b \in A \,/\, \{a^*\}\}$$

where $\#\{\bullet\}$ denotes the number of elements in the finite set $\{\bullet\}$. Assumption 3 implies that the number of voters who will support the candidate who offers $a^* \in \Omega$, when $A$ comprises the alternatives offered by the other candidates, is $s(a^*; A)$.

Note that we define the support by a strict inequality, thus ignoring all voters who are indifferent between two or more candidates (for example, when the voter's ideal point is exactly midway between two alternatives or when two or more candidates offer the same alternative). However, our nonexistence result is sufficiently robust, no matter how the indifferent voters are allocated because in our examples there might, in equilibrium, be at most one indifferent individual, who cannot be a decisive voter. The definition of support also implies that no two candidates will choose to offer the same alternative because we implicitly assume that each would then have an empty support. Finally, because candidates adopt distinct positions, our definition of support also enables us to identify a "running candidate" with the alternative that he or she offers. Hence, a set of alternatives $A$ is said to be a $K$-equilibrium if it consists of $K$ alternatives and if no other alternative, given $A$ and this additional alternative, is supported by more voters than any of the original alternatives in $A$ (thus, no potential candidates can guarantee themselves seats).

DEFINITION 2. *A set of alternatives $A$ is a* K-equilibrium *if (1) $A$ contains exactly $K$ alternatives; and (2) for all $b \in \Omega / A$,*

$$s(b; A \cup \{b\}) \leqslant s(a; A \cup \{b\}) \text{ for all } a \in A.$$

Condition 1 guarantees that the number of candidates elected coincides with the exogenously given size, $K$, of the legislature. Condition 2 implies that no potential candidate will find it worthwhile to offer some available alternative $b \in \Omega/A$ because the measure of support would not exceed that of the (resulting new) support of *any* of the $K$ candidates already running for office. Indeed, Condition 2 asserts that the measure of the support of *each* of the original running candidates will rank (weakly) among the top $K$.

The main result of the paper is the following:

THEOREM. *For every given $K \geqslant 2$, societies satisfying Assumptions 1–3 exist for which there is no K-equilibrium.*

## The Main Result for the Finite Case: Preliminaries

To prove this theorem for the finite case, we make use of the following:

LEMMA 1: *Let $K = 2$. Then there are finite societies satisfying Assumptions 1–3 for which no two-equilibrium exists.*

*Proof.* Consider the following example with 15 individuals whose peaks constitute the set of alternatives and who have to choose two candidates. Specifically,

$$\begin{array}{ll} K = 2 & \\ T = \{1, \ldots, 15\} & \\ q(t) = t & \text{for } t \in L \equiv \{1, 2, 3, 4\} \\ q(t) = 10 + t & \text{for } t \in C \equiv \{5, 6, 7, 8, 9, 10, 11\} \\ q(t) = 20 + t & \text{for } t \in R \equiv \{12, 13, 14, 15\} \\ \Omega = \{q(t)\}_{t \in T} & \end{array}$$

$L$, $C$, and $R$ stand for *left*, *center*, and *right*, respectively (see Figure 1).

Suppose, in negation, that there exists a two-equilibrium $A = \{x_1, x_2\}$. Without loss of generality, $x_1 < x_2$. We shall show that for any choice of $\{x_1, x_2\}$, there always exists $\bar{x} \in \Omega / A$ for which

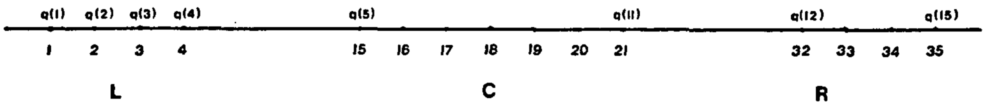
$$s(\bar{x}; A \cup \{\bar{x}\}) > s(x_j; A \cup \{\bar{x}\})$$

for at least one $j \in \{1, 2\}$, implying that $A$ is not a two-equilibrium. Distinguish among the following five cases:

1. $A \subset L$. Then, by choosing $\bar{x} = q(5)$, we have
   $$s(\bar{x}; A \cup \{\bar{x}\{) = 11 > s(x_j; A \cup \{\bar{x}\})$$
   for $j = 1, 2$
2. $A \subset R$. Then, by choosing $\bar{x} = q(11)$, we have the same situation as in Case 1 above
3. $A \subset C$. Then, for at least one $j \in \{1, 2\}$,
   $$|S(x_j; A) \cap C| \leqslant 3$$
   Without loss of generality, let this $j = 1$. Then, for $\bar{x} = q(4)$ we have
   $$s(\bar{x}; A \cup \{\bar{x}\}) = 4 > s(x_1; A \cup \{\bar{x}\})$$
4. $x_1 \in L$ and $x_2 \in R$. Then, by choosing $\bar{x} = q(8)$, we have
   $$s(\bar{x}; A \cup \{\bar{x}\}) = 7 > 4 = s(x_j; A \cup \{\bar{x}\})$$
   for $j = 1, 2$

Thus, the only two possibilities that remain to be checked are

Figure 1. Distribution of Ideal Points for Lemma 1



5. $[x_1 \in L$ and $x_2 \in C]$ or $[x_1 \in C$ and $x_2 \in R]$. In either case, for $\{h, j\} = \{1,2\}$,

$$s(x_h; A) = 4 \text{ and } s(x_j; A) = 11$$

Then, it is possible to choose $\bar{x}$ adjacent to $x_j$ so that

$$s(\bar{x}; A \cup \{\bar{x}\}) \geqslant 5 > 4 = s(x_h; A \cup \{\bar{x}\})$$

Q.E.D.

## The Main Result for the Finite Case: Proof

Let $K \geqslant 3$ be given. Define $T = \{1, \ldots, 7K + 1\}$ with ideal points distributed as follows:

$$q(t) = \begin{cases} t & \text{if } t \in L = \{1, 2, 3, 4\} \\ 10 + t & \text{if } t \in C = \{5, 6, 7, 8, 9, 10, 11\} \\ 20 + t & \text{if } t \in R = \{12, 13, 14, 15\} \\ 100j + t & \text{if } 2 + 7j \leqslant t \leqslant 8 + 7j, \\ & (j = 2, \ldots, K - 1) \end{cases}$$

and $\Omega \equiv \{q(t)\}_{t \in T}$

That is, $\{q(1), \ldots, q(15)\}$ are distributed as in the proof of Lemma 1. The remaining $q(t)$s, $t = 16, \ldots, 7K + 1$, are distributed evenly in $K - 2$ groups of seven, each group displaced by a very large distance from the previous group.

CLAIM 1. *In equilibrium, each of the $K - 2$ groups of seven voters must have at least one candidate choosing a strategy in the range of its ideal points.*

*Proof.* Assume the contrary. Then at least one group has no candidate in its range. A potential entrant guarantees himself or herself seven votes by entering there. The existing $K$ candidates share the remaining $7K - 6$ votes. On average this amounts to $(7K - 6)/K$, which is less than seven, so at least one existing candidate receives fewer votes than the entrant. Thus, the original $K$ candidate positions could not have been an equilibrium.

A consequence of Claim 1 is that there are *at most* two candidates in the range of $L \cup C \cup R$.

CLAIM 2. *In equilibrium, there is at least one candidate in the range of $L \cup C \cup R$.*

*Proof.* Assume the contrary, namely that no candidate locates in $L \cup C \cup R$. Then an entrant, locating at $q(15)$, receives 15 votes, and the original $K$ candidates share the remaining $7K - 14$ votes. At least one of these must receive fewer than seven votes, so entry is successful and the positions of the original $K$ candidates could not have been an equilibrium.

CLAIM 3. *In equilibrium, there are at least two candidates in the range of $L \cup C \cup R$.*

*Proof.* From Claim 2 there is at least one, so suppose, contrary to the hypothesis of Claim 3, that there is exactly one. An entrant may choose a strategy in $L \cup C \cup R$ that assures him or her of at least seven votes. The $K - 1$ candidates not in $L \cup C \cup R$ share $7K - 14$ votes. But $7K - 14 = 7(K - 1) - 7 < 7(K - 1)$, so at least one of these candidates receives fewer than seven votes. Entry is successful, so the circumstance with exactly one candidate in $L \cup C \cup R$ cannot be an equilibrium.

Claims 1, 2, and 3 imply that in equilibrium there must be exactly two candidates in $L \cup C \cup R$ (and $K - 2$ candidates in the $K - 2$ remaining groups). But this means that there must be a two-equilibrium in $L \cup C \cup R$, an impossibility by Lemma 1. This establishes the main result for the finite case.

## Proof of the Main Result for the Continuum Case: Preliminaries

In order to establish our main result for the continuum case, we use two lemmas.

LEMMA 2. *Consider a society* $(\Omega, F)$ *where* $\Omega$ *is some closed interval in R, say* $\Omega = [\alpha, \beta]$ *with* $\alpha < \beta$, *and let F be a continuous distribution function over* $\Omega$. *Assume that* $A = \{x_1, x_2\}$, $x_1 < x_2$, is a two-equilibrium for $(\Omega, F)$. *Denote* $\gamma \equiv (x_1 + x_2)/2$. *Then,*

$$F(x_1) = ½\, F(\gamma) \text{ and } F(x_2) = [1 + F(\gamma)]/2 \qquad (1)$$

and

$$s(x_i; A) \geqslant ½\, s(x_j; A), \{i, j\} \equiv \{1, 2\} \qquad (2)$$

A formal proof of this lemma, a straightforward application of the median-voter result, will be found in the Appendix.

LEMMA 3. *Let* $K = 2$. *Then there are continuous societies,* $(\Omega, F)$, *for which no two-equilibrium exists.*

*Proof of Lemma 3.* Let $\Omega = [0, 10]$ and let $F$ satisfy $F(1) = F(4) = .3$ and $F(6) = F(9) = .7$. That is, each voter belongs to one of the three classes: *left* (those whose ideal points lie in [0, 1]), *center* (those whose ideal points lie in [4, 6]), and *right* (those whose ideal points lie in [9, 10]). These classes contain, respectively, 30%, 40%, and 30% of the population. We shall show that no two-equilibrium exists for such a society. Assume in negation, that $A = \{x_1, x_2\}$, $x_1 < x_2$, is a two-equilibrium, and again let $\gamma \equiv (x_1 + x_2)/2$. Distinguish between the following two cases:

1. $x_1 \leqslant 1$. If $x_2 > 6$, letting $\bar{x} \equiv 6$, we have $S(\bar{x}; A \cup \{\bar{x}\}) \subset [4, 6]$, and therefore $x(\bar{x}; A \cup \{\bar{x}\}) \geqslant .4$ while $s(x_i; A \cup \{\bar{x}\}) \leqslant .3$, $i = 1, 2$. Hence, $A$ cannot be a two equilibrium.

   If $x_2 \leqslant 6$, then $S(x_2; A) \subset [4, 10]$, implying $½\, s(x_2; A) \geqslant .35 > .3 \geqslant s(x_1; A)$, which contradicts Condition 2 of Lemma 2.
2. $x_1 > 1$. Then $F(x_1) \geqslant F(1) = .3$ which, from Condition 1 of Lemma 2 implies $F(\gamma) \geqslant .6$. Using Condition 1 again, it follows that $F(x_2) = ½ + ½\, F(\gamma) \geqslant .8$. Therefore, $x_2 \geqslant 9$.

   If $x_1 < 4$, then for $\bar{x} \equiv 4$, we have $S(\bar{x}; A \cup \{\bar{x}\}) \subset [4, 6]$, and therefore $S(\bar{x}; A \cup \{\bar{x}\}) \geqslant .4 > .3 \geqslant s(x_i; A \cup \{\bar{x}\})$, $i = 1, 2$, contradicting the fact that $A$ is a two-equilibrium.

   If $x_1 \geqslant 4$, then $S(x_1; A) \subset [0, 6]$, implying that, $½\, s(x_1; A) \geqslant .35 > .3 \geqslant s(x_2; A)$, which contradicts Condition 2.

This example establishes the lemma for the continuum case.

Q.E.D.

## Main Result for the Continuum Case: Proof

Let $K \geqslant 3$ be given. Define

$$\Omega = [0, 110(K - 2) + 10]$$

and let $F$ be given by:

$$F(1) = F(4) = 2/K\ (0.3)$$

$$F(6) = F(9) = 2/K\ (0.7)$$

$$F(10) = F(110) = 2/K$$

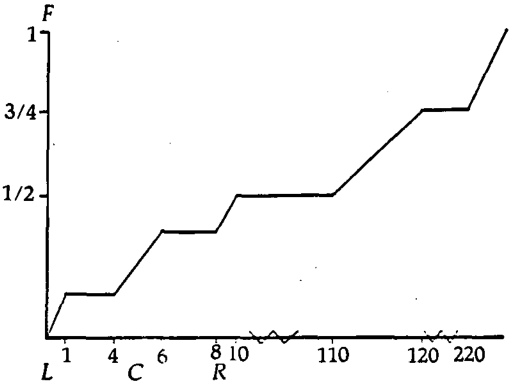
$$F(120) = F(220) = 3/K$$

$$\vdots$$

$$F(10 + (K - 3) \cdot 110)$$
$$= F[(K - 2) \cdot 110] = (K - 1)/K$$

An example of this distribution function is displayed for $K = 4$ in Figure 2.

**Figure 2. Distribution Function $F$ for $K = 4$**



Generally, $2/K$ of the voters are distributed over [0, 10] and then, with gaps of one hundred scale units, $1/K$ of the voters are distributed over each of $(K-2)$ intervals of length 10.

Using arguments similar to those employed in the proof of Lemma 1, the reader can verify that if a $K$-equilibrium exists, then at most two candidates can occupy locations in the [0, 10] interval.

CLAIM 1. *There is no $K$-equilibrium if there is no candidate in [0, 10].*

*Proof.* Suppose the contrary and let an entrant enter at 10. His or her measure of support is $2/K$. The existing $K$ candidates divide support measuring $(K-2)/K$, that is, these $K$ candidates have average support measuring $(K-2)/K^2$. But $2/K > (K-2)/K^2$, so entry is successful.

CLAIM 2. *One candidate in [0, 10] is not a $K$-equilibrium.*

*Proof.* Suppose the contrary. Then a second candidate could enter in [0, 10] and be assured of a measure of support at least as large as $1/K$. The $(K-1)$ candidates not in [0, 10] have support measuring $(K-2)/K$. So the entrant is successful because $1/K > (K-2)/K(K-1)$.

CLAIM 3. *Two candidates in [0, 10] is not a $K$ equilibrium.*

*Proof.* This claim is precisely the example in the proof of Lemma 3.

The three claims are inconsistent with the hypothesis of at most two candidates in [0, 10], thereby establishing that no equilibrium for the $(\Omega, F)$ given above exists.

Q.E.D.

## Six Remarks

1. The theorem cannot be strengthened to include the case of $K = 1$. Indeed, Assumptions 1–3 imply Black's (1958) median-voter theorem, so that the median of $F$ is always a one-equilibrium.
2. For any fixed finite society of size $T$, where individuals have distinct ideals, there will always be a unique $T$-equilibrium, namely $\{q(1), \ldots, q(T)\}$. There will also always be a $(T-1)$-equilibrium, namely the collection $\{q_{-i}(t)\}$ of all ideals but the $i$th. Finally, there will also be a $(T-2)$-equilibrium, with the collection $\{q(2), \ldots, q(T-1)\}$ being one such instance. None of these instances contradicts the theorem proved above because we may always construct a society (of some size $T^* \neq T$) for which a $T$-equilibrium does not exist.
3. Because for every given society a one-equilibrium exists (see Remark 1), a society choosing $K$ legislators, by first partitioning itself into $K$ constituencies (geographically, racially, ethnically, etc.) and then empowering each constituency to choose a single representative, produces an equilibrium (in fact, $K$ one-equilibria), whereas that same society choosing $K$ legislators "at large" would not in general produce an equilibrium.
4. Assumptions 1–3 are especially strin-

gent requirements, suggesting that our negative result is quite robust. Our results imply, for any $K > 1$, that $K$-equilibria might fail to exist if the space of alternatives were multidimensional (relaxing Assumption 1) or if voter preferences are nonsymmetric or non-single-peaked (relaxing Assumption 2).

5. One consequence of the nonexistence of a $K$-equilibrium is that at the next election there may be a successful entrant and, therefore, at least one defeated incumbent, even though $F$ remains unchanged. It is therefore entirely possible for legislative policies to shift around under this representative institution even if individual preferences do not.
6. Our definition of a $K$-equilibrium describes the locations of *winning* positions. Candidates (both actual and potential entrants) in this definition do not care about the measure of their support so long as no more than $K - 1$ other candidates have greater measure. An actual candidate is satisfied as long as this is true, and a potential entrant is deterred so long as the potential support is always weakly exceeded by that of the $K$ other candidates. Our approach says nothing about "losers" other than that, if winning (finishing among the top $K$) is their sole objective, they will be deterred from entering. Of course, there may be many reasons to enter a contest besides the hope of "winning." By finishing $(K + 1)$th, for example, a lawyer-candidate's private practice may be enhanced, or his or her chances of winning next time may be improved. In general, if there are rewards like these attached to the top $K + L$ ranks, then the appropriate equilibrium (with some additional complications) is not a $K$-equilibrium but rather a $(K+L)$-equilibrium. That is, the reasons for running, over and above election to office, should be explicitly incorporated. On the other hand, if no such rewards are attached to ranks $(K + 1), \ldots, (K + L)$, then in a $K$-equilibrium there should be no entry by "losers."

## Limit Equilibrium

Recently, Palfrey (1984) examined equilibrium in a unidimensional spatial context with entry. In his model, there are two "dominant" parties (or candidates) and a potential entrant competing for a single seat. Unlike the familiar Black-Downs model, in which two parties converge to the median of the distribution of voters, the Palfrey model, by allowing the prospect of third-party entry, destroys the median equilibrium and pulls the dominant parties apart.

Specifically, Palfrey accepts Assumptions 1–3 given above and defines $\Omega = [0, 1]$ with voter ideals distributed on $\Omega$ according to $F$ that satisfies

$$F(0) = 0, F(1) = 1, \tag{3}$$

$$F(x) = 1 - F(1 - x) \text{ for } x \in \Omega \text{ (symmetry)}, \tag{4}$$

and

$F(\cdot)$ is continuous, strictly increasing, and twice differentiable on $\Omega$ such that

$$F''(x) \geq 0 \quad \text{if } x < 1/2 \text{ and} \tag{5}$$

$$F''(x) \leq 0 \quad \text{if } x > 1/2 \tag{6}$$

The last two requirements restrict $F$ to be convex in $[0, 1/2)$ and concave in $(1/2, 1]$, respectively, so that it takes on a familiar $S$ shape.

*He further assumes that all parties are vote maximizers,* so that the entrant always enters at its vote-maximizing location (whether it can win or not). Thus, each of the dominant parties, which simultaneously choose locations prior to the entrant's choice, makes its spatial choice taking both the other dominant party's (simultaneous) choice and the entrant's (subsequent) choice into account. In effect, "the two established parties are engaged in a Cournot-Nash game with each other, but both are Stackelberg leaders with respect to the entrant" (Palfrey 1984, 140).

For the two dominant parties under the above assumptions, Palfrey establishes the following equilibrium (which he calls a "limit equilibrium"):

THEOREM. *$(\bar{x}_1, x_2)$ is a unique limit equilibrium, where*

$$F(\bar{x}_1) = 1 - 2F(1/4 + \bar{x}_1/2) \text{ and} \tag{7}$$

$$F(\bar{x}_2) = 1 - F(\bar{x}_1) \tag{8}$$

Moreover, he proves that $\bar{x}_1 \neq \bar{x}_2$ and that at $(\bar{x}_1, \bar{x}_2)$, the entrant never wins. The latter fact allows him to claim that "two party systems are stable, in the sense that if the initial state of the system is that there are two established parties who rationally anticipate the entry decision of a single vote-maximizing entrant, then the two established parties will remain *established* parties" (Palfrey 1984, 153).

The Palfrey limit equilibrium bears a resemblance to our two-equilibrium. Yet, in contrast to our theorem, Palfrey proves existence (and uniqueness). We explore the differences in the two equilibrium notions in the context of a concrete example where both exist. We then discuss the appropriateness of these two equilibrium concepts.

We shall say that $(x_1, x_2)$ is a *symmetric* two equilibrium if $x_1 + x_2 = 1$. In view of Condition 4, Condition 8 implies that Palfrey's equilibrium is symmetric.

*Example.* Let $\Omega = [0, 1]$ and define $F$ by

$$\begin{array}{ll} F(x) = 2x^2 & 0 \leq x \leq 1/2 \\ F(x) = 1 - F(1 - x) & 1/2 < x \leq 1 \end{array}$$

Note that $F(\cdot)$ satisfies Conditions 3–6.

For this example, we have

CLAIM 1. *There exists a unique symmetric two equilibrium, $(x_1^*, x_2^*)$. where $x_1^* = 1/2\sqrt{2}$ and $x_2^* = 1 - 1/2\sqrt{2}$.*

*Proof.* See Appendix.

CLAIM 2. *The unique symmetric limit equilibrium $(\bar{x}_1, \bar{x}_2)$ is given by $\bar{x}_1 = (1/6)(\sqrt{10} - 1)$ and $\bar{x}_2 = 1 - \bar{x}_1$.*

*Proof.* See Appendix.

Thus, in the above example, there is both a unique symmetric two-equilibrium and a unique symmetric limit equilibrium, but they are not the same. This then raises the central issue of what it is these competing concepts elucidate and whether either has a comparative theoretical advantage. We shall not tackle this problem generally here but will note some features that enter into a fuller consideration.

Although a limit equilibrium always exists in the domain described by Assumptions 1–3 and Conditions 3–6, entry is *never* deterred. Palfrey's entrant will always enter at $\gamma$, the median of $F$, because this is the vote-maximizing location when the established parties adopt the limit-equilibrium positions $(\bar{x}_1, \bar{x}_2)$. Moreover, it has been established by Palfrey that

$$s(\gamma; \gamma, \bar{x}_1, \bar{x}_2) < s(\bar{x}_i; \gamma, \bar{x}_1, \bar{x}_2)\ i = 1, 2$$

so that the entrant finishes *last* at its vote-maximizing location. Hence, Palfrey concludes that two-party systems are "stable" and established parties will remain "established."

But there would appear to be an inconsistency here, for if all the facts in the preceding paragraph are true, one must wonder why an entrant would bother entering at all. Or, to put the question positively, could a potential entrant improve its political fortunes by pursuing an objective other than vote maximization?

Suppose, instead, that a potential entrant in a system of two established parties seeks not to maximize votes (which, if correctly anticipated by the established parties, ensures that the entrant will finish last) but rather *to displace one of the established parties.* That is, suppose an entrant sought to finish among the top two parties in order to become, itself, one of the established parties. In fact, if the established parties locate at Palfrey's limit-equilibrium $(\bar{x}_1, \bar{x}_2)$, then an entrant motivated to displace can succeed by locating either just to the left of $\bar{x}_1$ or just to the right of $\bar{x}_2$ because, as can be easily verified,

$$1/2 - F(\bar{x}_1) < F(\bar{x}_1) \text{ and } 1 - F(\bar{x}_2) > F(\bar{x}_2) - 1/2$$

If, on the other hand, established parties anticipate this objective and if a two-equilibrium $(x_1^*, x_2^*)$ exists, then no entry will occur. The established parties will remain established because at $(x_1^*, x_2^*)$ they each guarantee finishing among the top two even if entry should occur. In fact, this is what a two-equilibrium insures.

## Conclusion

This discussion raises a fundamental question about candidate objectives. As we noted earlier, objectives are system-dependent. If the route to office in a system requires a party to become one of two "established" parties (which presumably rotate in office, ceteris paribus), then a third-party entrant would be ill advised, by Palfrey's own theorem, to seek to maximize votes. As Table 1 and our earlier discussion suggest, in the absence of a two-equilibrium, a third-party *rank maximizer* would seem better positioned to obtain office (if not in this election, then in the next, after it has displaced one of the major parties). Only if a two-equilibrium exists will the third party's objective be frustrated, and its entry deterred. Lacking existence of a two-equilibrium, a third party may wreak havoc.

Consequently, we regard the deterrence property of the two-equilibrium concept to be a desirable feature of any equilibrium concept in which entry is possible. Our nonexistence result, in establishing that a two-equilibrium ($K$-equilibrium) cannot be assured in even a fairly restrictive domain, illuminates the potential for entry and the consequent poor prospects for "established" parties to remain established—a fact obscured by the limit-equilibrium concept. This, in turn, suggests that there will often be a strategic tension in multiparty systems: each established party is torn between competing against its established opponents and protecting its flanks against potential entrants trying to displace it. Our theorem shows that these two objectives cannot always be pursued simultaneously when entry is "free."

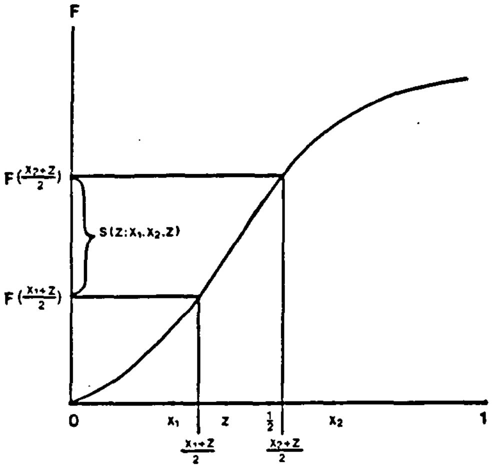
One avenue well worth exploring, therefore, is a theory of entry barriers in which established parties make it costly for potential entrants to participate. One such barrier may stem from "reputational effects," according to which established parties are advantaged in their spatial choices relative to potential entrants because the latter lack credible reputations for performance.

Finally, while our main result establishes that a $K$ equilibrium cannot be guaranteed by Assumptions 1–3 for all $F$, it is of interest to establish restricted domains for $F$ in which such equilibria do exist.

## Appendix

*Proof of Lemma 2.* Condition 1 is an immediate corollary of the median voter result, because it states that $x_1$ is the median in $[\alpha, \gamma]$ and that $x_2$ is the median in $[\gamma, \beta]$. In order to establish Condition 1 as a necessary property of a two-equilibrium, we need to rule out the following four strict inequalities: (1) $F(x_1) < [F(\gamma)]/2$, (2) $F(x_1) > [F(\gamma)]/2$, (3) $F(x_2) < [1 + F(\gamma)]/2$, and (4) $F(x_2) > [F(\gamma)]/2$.

Figure A-1. Measure of Support of Entrant



1. Assume that $F(x_1) < [F(\gamma)]/2$. Let $x \in [x_1, \gamma]$ be such that $F(x_1) < F(x) < [F(\gamma)]/2$ (such an $x$ exists by the continuity of $F$). Then,

$$s(x_1; A \cup \{x\}) = F\left(\frac{x_1 + x}{2}\right) \leq F(x)$$
$$< \tfrac{1}{2} F(\gamma), \text{ and}$$
$$s(x; A \cup \{x\}) = F\left(\frac{x + x_2}{2}\right) - F\left(\frac{x + x_1}{2}\right)$$
$$\geq F(\gamma) - F(x) > F(\gamma) - \tfrac{1}{2} F(\gamma) = \tfrac{1}{2} F(\gamma)$$

   But the above two inequalities contradict the fact that $A$ is a two-equilibrium, because the support of $x$ exceeds the support of $x_1$.

2. Assume that $F(x_1) > [F(\gamma)]/2$. Let $x \in [\alpha, x_1]$ be such that $[F(\gamma)]/2 < F(x) < F(x_1)$ (again, such an $x$ exists by the continuity of $F$). Then,

$$s(x_1; A \cup \{x\}) = F(\gamma) - F\left(\frac{x + x_1}{2}\right)$$
$$\leq F(\gamma) - F(x) < \tfrac{1}{2} F(\gamma) \text{ and}$$
$$s(x; A \cup \{x\}) = F\left(\frac{x + x_1}{2}\right) \geq F(x)$$
$$> \tfrac{1}{2} F(\gamma)$$

   But then, once again, $A$ cannot be a two-equilibrium.

The other two cases, 3 and 4, are ruled out by similar arguments, thereby establishing Condition 1.

In order to prove Condition 2, we have to show that neither $s(x_2; A)$ $[s(x_1; A])/2$ nor $s(x_1; A) < [s(x_2; A)]/2$ holds.

Assume that $s(x_2; A) < [s(x_1; A)/2$. Then, by Condition 1, $s(x_2; A) < F(x_1)$. Let $x \in [\alpha, x_1]$ be such that $F(x_1) > F(x) > s(x_2; A)$. For such an $x$,

$$s(x; A \cup \{x\}) \geq F(x) > s(x_2; A) = s(x_2; A \cup \{x\})$$

Hence, $A$ cannot be a two-equilibrium. In a similar way, the second inequality is also ruled out, implying that Condition 2 holds. This establishes Lemma 2.

*Proof of Claim 1.* First note that if $(x_1, x_2)$ is a symmetric two equilibrium, then $F(x_1) = 1/4$ and $F(x_2) = 3/4$. Indeed, from symmetry of $(x_1, x_2)$, $\gamma = [x_1 + x_2]/2 = 1/2$. From Condition 4, we therefore have $F(\gamma) = 1/2$. But, from Condition 1, we know that $F(x_1) = [F(\gamma)]/2 = 1/4$, and from the symmetry of $(x_1, x_2)$, it follows that $F(x_2) = 3/4$. From the definition of $F(\bullet)$ in the example, we conclude that $x_1 = 1/2\sqrt{2}$ and $x_2 = 1 - 1/2\sqrt{2}$.

We now show that $(1/2\sqrt{2}, 1 - 1/2\sqrt{2})$ is in fact a symmetric two-equilibrium. Without loss of generality, suppose an entrant ($z$) enters in $[x_1, 1/2]$. Define $L(z)$ to be the difference in votes between the entrant and the candidate located at $x_1$. If $L(z) > 0$, then $(x_1, x_2)$ is not a two-equilibrium since $z$ will have received more votes than $x_1$. With the assistance of Figure A-1, it may be seen that the measure of support of $z$ given $B$ is given by

$$s(z; B) = F\left(\frac{x_2 + z}{2}\right) - F\left(\frac{x_1 + z}{2}\right)$$

where $B = \{x_1, x_2, z\}$. But the measure of support of the candidate at $x_1$ is $s(x_1; B) = F[(x_1 + z)/2]$. Thus,

$$L(z) = F\left(\frac{x_2 + z}{2}\right) - 2F\left(\frac{x_1 + z}{2}\right).$$

Because $[x_2 + z]/2 > 1/2$ (as $z > x_1$ and $[x_1 + x_2]/2 = 1/2$ from symmetry), the first term of $L(z)$ is given by

$$F\left(\frac{x_2 + z}{2}\right) = F\left(\frac{1 - x_1 + z}{2}\right) = 1$$
$$- F\left(1 - \frac{1 - x_1 + z}{2}\right) = 1 - 2\left(1 - \frac{1 - x_1 + z}{2}\right)^2$$
$$= 1 - 1/2\left(\frac{1 + x_1 - z}{2}\right)^2$$

Similarly,

$$F\left(\frac{x_1 + z}{2}\right) = 2\left(\frac{x_1 + z}{2}\right)^2 = 1/2\,(x_1 + z)^2$$

and so

$$L(z) = 1 - 1/2\,(1 + x_1 - z)^2 - (x_1 + z)^2$$

Notice first that $L(x_1) = 0$ for $x_1 = 1/2\sqrt{2}$ and second that $[dL(z)]/dz = 1 - x_1 - 3z$. For $x_1 = 1/2\sqrt{2}$ and $z \geqslant x_1$, simple arithmetic establishes that $[dL(z)]/dz < 0$. These two facts imply $L(z) \leqslant 0 \ \forall\ z \in [x_1, 1/2]$.

Thus, $z$ cannot win more votes than $x_1$ by entering in $[x_1, 1/2]$. A symmetric argument demonstrates that $z$ cannot win more votes than $x_2$ by entering in $[1/2, x_2]$. Finally, because $x_1$ is the median of $[0, 1/2]$, that is, $F(x_1) = 1/4$, and $x_2$ is the median of $[1/2, 1]$, that is, $F(x_2) = 3/4$—both facts established in the claim above—no entrance in $[0, x_1]$ or $[x_2, 1]$ will succeed. Therefore, $(x_1, x_2) = (1/2\sqrt{2}, 1 - 1/2\sqrt{2})$ is a two-equilibrium.

Q.E.D.

*Proof of Claim 2.* Let $(\bar{x}_1, \bar{x}_2)$ be a limit equilibrium which we know exists and is unique. Then, because $\bar{x}_1 \leqslant \bar{x}_2$, $z = x_1 \leqslant 1/2$. Therefore, $1/4 + z/2 \leqslant 1/2$. Hence, $F(z) = 2z^2 = 1 - 2[2(1/4 + z/2)^2]$, implying $3z^2 + z - 3/4 = 0$. The unique nonnegative root of this quadratic equation is $z = 1/6(\sqrt{10} - 1)$.

## Note

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# STABILITY AND CENTRALITY OF LEGISLATIVE CHOICE IN THE SPATIAL CONTEXT

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*Majority-rule spatial voting games lacking a core still always present a "near-core" outcome, more commonly known as the* Copeland winner. *This is the alternative that defeats or ties the greatest number of alternatives in the space. Previous research has not tested the Copeland winner as a solution concept for spatial voting games without a core, lacking a way to calculate where the Copeland winner was with an infinite number of alternatives. We provide a straightforward algorithm to find the Copeland winner and show that it corresponds well to experimental outcomes in an important set of experimental legislative voting games. We also provide an intuitive motivation for why legislative outcomes in the spatial context may be expected to lie close to the Copeland winner. Finally, we show a connection between the Copeland winner and the Shapley value and provide a simple but powerful algorithm to calculate the Copeland scores of all points in the space in terms of the (modified) power values of each of the voters and their locations in the space.*

It is well known that, under majority rule in a world with more than one issue dimension, a majority winner does not, in general, exist. That is, there is no one alternative that can defeat each and every other alternative in paired contest (McKelvey 1976, 1979; Schofield 1978). Furthermore, we can expect that all or virtually all the alternatives will cycle with one another. One common interpretation of those results is that institutions that use majority rule ought not to work: since choices are cyclical, losers should always be able to find some alternative they like better that could defeat the present status quos, and so on ad infinitum. Thus, all legislatures should be in constant turmoil as losers try to reverse decisions they do not like. Another implication of the results of what are often called "chaos" theorems is that expected outcomes of legislative majority-rule decision processes could range over the entire issue space, depending upon exactly how the agenda is specified; hence, political decision making should lack any clear central tendency. These instability results have led to considerable skepticism about the usefulness of the standard populist view of democratic decision making as a clear choice from among a set of alternatives in accord

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with the will of the majority (see review in Riker 1980, 1982).

When we look at the world, whether it be the real world of legislative decision making or the parallels to that real world that have been generated by carefully constructed experimental voting games such as those of Fiorina and Plott (1978), we find that outcomes are actually quite stable and that voters in a committee or legislative setting are able to pick an alternative and stick to it. Indeed, in legislatures, once a bill is enacted, changes from a given status quo are apt to be only incremental, even over a period of years. Also, outcomes are far from randomly distributed over the issue space. Rather, votes seem to track some central tendency in the group. In particular, in experimental legislative spatial voting games where, in principle, *any* outcome would be feasible, we observe that different committees rapidly make choices that are remarkably alike (see, e.g., Fiorina and Plott 1978).

Investigating why the world does not behave as the theoretical models of majority rule in more than one dimension say it should has been a booming industry in the past several years (Tullock 1981). Various approaches have been offered to account for an observed stability in legislative majority-rule processes beyond what could be expected by the various "chaos" theorems and impossibility results. Among these is the view that various institutional features simplify or delimit the domain of choice so as to make a majority winner if not certain, at least much more likely (Grofman and Feld 1986; Shepsle 1979; Shepsle and Weingast 1982). Also, even though there may not be a unique majority winner and even though the top cycle set (i.e., the minimal set of alternatives that cycle with one another) may be very large, there may still be a probability distribution over expected outcomes that is well behaved, for example, with probability mass concentrated in some central area of the Pareto set (Ferejohn, Fiorina, and Packel 1980; Ferejohn, McKelvey, and Packel 1984). Moreover, even if prior to the actual decision there were alternatives that could have defeated it in pairwise contest, a number of postdecisional mechanisms (both cognitive and institutional) act to reinforce the legitimacy and stability of whatever choice the society may have made (Grofman and Uhlaner 1985).

A variety of other ideas have been proposed to account for stability. For example, it has been suggested that the de facto decision process may be supramajoritarian and this may lead to stability even in situations where the majority rule is unstable (Schofield, Grofman, and Feld 1985). Kramer (1977) argues that repeated two-party competition will drive outcomes into the minimax set in the interior of the Pareto set. Wuffle, Feld, Owen, and Grofman (n.d.) argue the attractiveness of what they label the "finagle point" in spatial games of two-candidate competition. A number of authors claim that coalition-formation processes will induce stability, proposing a plethora of competing solution concepts as to where outcomes will lie (see, e.g., McKelvey, Ordeshook, and Winer 1978; Owen 1982; Straffin and Grofman 1984). It has also been argued that stability is more likely if actual choices more nearly resemble a yes-no referendum on whether existing policies will be maintained than if choices are made among a numerous and almost certainly cyclic set of alternatives (Riker 1982). Finally, Miller (1980) and others (Banks 1985; Feld, Grofman, and Miller 1985; McKelvey 1986; Miller, Grofman, and Feld 1985; Moulin 1984; Shepsle and Weingast 1984) have suggested that, even when all alternatives cycle with one another, there may be sufficient internal structure to the majority-preference relation for a legislative or polity that the only likely outcomes either will be few or will lie in a small area central in the issue space, within the set of outcomes that has been called the "uncovered set."

In exploring the reasons for stability of legislative choice, we focus on the properties in the spatial context of an old solution concept, the *Copeland winner.* The *Copeland winner* (which we shall also refer to as the *strong point*) is simply that alternative which is majority-preferred to the highest proportion of other alternatives in the space. The Copeland winner is identical to the core if a majority winner exists. The core is the most theoretically powerful and empirically predictive of known solution concepts. The Copeland winner has never before been applied to try to make sense of multidimensional legislative voting because it was not known to calculate where the Copeland winner was in a multidimensional issue space with an infinite number of alternatives.

The aim of this paper is threefold. First and foremost, we show that the Copeland winner, known in the finite case to have a number of desirable axiomatic characteristics (Copeland 1951; Straffin 1980), has a number of additional properties that make it a particularly attractive solution concept in the spatial context. In the spatial context, the Copeland winner (1) always exists, will in general be unique, and can be located relatively easily; (2) once found, turns out to predict outcomes in the Fiorina and Plott (1978) experimental voting games better than do more than one dozen previously proposed models; and (3) can be given an intuitive motivation that seems to explain why voters in a legislative situation would end up with outcomes at or near the Copeland winner.

Second, we show that, in the spatial context, the Copeland winner can be expressed in terms of the (modified) Shapley value, a key game-theoretic measure of power. This connection is a surprising one and important in that it shows a deep mathematical link between two hitherto largely unrelated areas of research.

Finally, we show that the Copeland values (defined as the proportion of all other points to which a given point is majority-preferred) fall off monotonically with distance from the Copeland winner along any ray. Moreover, the strong point is always in the uncovered set[1] and (roughly speaking) a central point in the Pareto set. Hence, the most "popular" alternatives in the space are all spatially clustered near a central area of the Pareto set.

We believe that most legislative agenda processes can be expected to lead voters to outcomes near the strong point,[2] especially in legislative voting games played noncooperatively (i.e., without coalitional alliances) and without party organizations to structure voting.

## Locating the Strong Point

Finding the strong point requires finding a point $x$ such that the area (measure) of the set of points that are majority-preferred to it is minimal. We shall present the basic results for two dimensions: for Euclidean preferences; and for $n$, the number of voters, odd.

THEOREM 1. *For voter ideal points located on a convex polygon, the strong point, $x$, is given by*

$$x = \frac{\sum_{j=1}^{n} c_j P_j}{\pi} \qquad (1)$$

*where the $P_j$ are the voter ideal points, and the $c_j$ correspond to the "star" angles of the polygon (measured in radians).*

A complete proof of this result is given in Shapley and Owen 1985. We illustrate the calculations for $n = 5$ (see Figure 1).

Figure 1. The Star Angles $c_1$ through $c_5$ of a Convex Pentagon

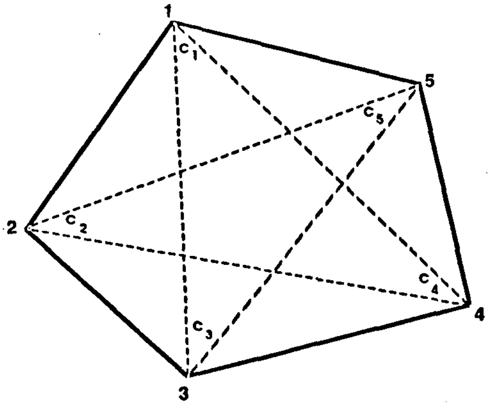
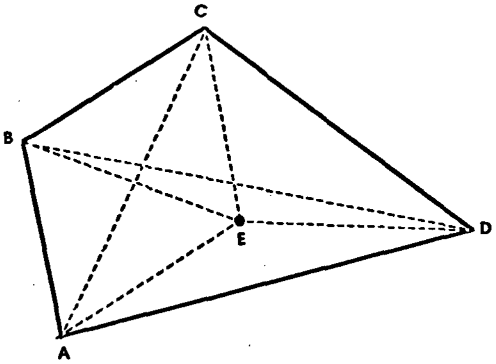


Figure 2. Five-Voter Game with One Ideal Point, *E*, inside the Convex Hull (*E* Also Being the Strong Point)



The star angles of the pentagon shown in Figure 1 are

$$c_1 = \sphericalangle 413 = .4636$$
$$c_2 = \sphericalangle 524 = .8663$$
$$c_3 = \sphericalangle 135 = .8330$$
$$c_4 = \sphericalangle 241 = .2408$$
$$c_5 = \sphericalangle 352 = .7378$$

The sum of the star angles, $\sum_{j=1}^{n} c_j$, equals $\pi$.

The $P_1$ values (locations of voter ideal points) in Figure 1 are given by

$P_1 = (80, 40)$
$P_2 = (100, 40)$
$P_3 = (140, 80)$
$P_4 = (120, 110)$
$P_5 = (60, 100)$

Substituting these values in Equation 1, we obtain $x = (99.79, 70.06)$.

When not all voter ideal points are on a convex polygon, the minimization formula is identical, but the specification of the angles changes. For example, for the five-voter game with one voter, *E*, inside the convex hull (see Figure 2), the strong point is given by the same formula as in Equation 1, but now $A = (30, 52)$, $B = (25, 72)$, $C = (62, 109)$, $D = (165, 32)$, $E = (51, 59)$. Then

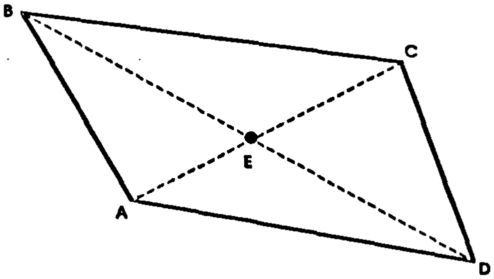
$$c_1 = c_A = \sphericalangle CAE = .7375$$
$$c_2 = c_B = \sphericalangle EBD = .1853$$
$$c_3 = c_C = \sphericalangle ACE = .2950$$
$$c_4 = c_D = \sphericalangle BDE = .0457$$
$$c_5 = c_E = \sphericalangle BEC + \pi - \sphericalangle AED = 1.878$$

Thus, $x = (47.23, 62.43)$.

Thus, in two dimensions, locating the strong point is quite straightforward.

If there is a core, it is well known that with an odd number of voters the core must be one of the voter ideal points

Figure 3. Five-Voter Game with a Core



(McKelvey and Wendell 1976). Plott (1967) provides a condition sufficient for a core: the existence of an (interior) point, $E$, such that every line through that ideal point passes through the same number of voter ideal points on each side of $E$. We show in Figure 3 a five-voter situation in which the ideal points satisfy the Plott conditions. Here, $c_1$ through $c_4$ would be zero and $\measuredangle BEC = \measuredangle AED$; that is, $c_5 = c_E = \Pi$, and thus $x = P_5 = P_E$. Hence, $x$ is a core point.

The formula given in Equation 1 was derived for the case where all points in the space are feasible alternatives. However, this strong assumption is not required. Moreover, an adaptation of the methodology that gave rise to Equation 1 can be used to find the strong point for choice over any feasible set of alternatives, although the algebra may become more complex and numerical approximation techniques may be necessary. As long as the feasible set includes the points in a circle containing the central portion of the Pareto set, the formula given in Equation 1 will hold.[3] We subsequently show how to calculate the point with highest Copeland value for an important special case, that in which the feasible set of outcomes is restricted to the set of points that can defeat the status quo.

## The Spatial Distribution of Copeland Values: Simulation and Results and Implications for Agenda Processes

### Simulation Results

We use a simulation to determine the Copeland values for each of the points in the space for some simple five-voter games in two-dimensional space (games investigated by Ferejohn, Fiorina, and Packel 1980; and Fiorina and Plott 1978), one with a core and one without. Inspection of the resulting graphs shows the rate at which Copeland values drop off as we move away from the strong point.[4] Shapley and Owen (1985) have demonstrated that Copeland values will fall off monotonically with distance from the strong point along any ray. In particular, the isovalue curves form a family of concentric circles with the equation

$$(x - x^*)^2 + (y - y^*)^2 = k \qquad (2)$$

where $(x^*, y^*)$ is the strong point.

Our first example is one of the three five-voter games used in Fiorina and Plott 1978, game 1, with voter ideal points at (39, 68), (30, 52), (25, 72), (62, 109), and (165, 32). The core is at (39, 68) (see Figure 4). As we see, Copeland scores are very high near the core point. The experimental outcomes found by Fiorina and Plott in their high-payoff-with-communication condition had a mean of (37, 68); in their high-payoff-no-communication condition, the mean was (38, 69). In both conditions, but especially the former, agreed-upon outcomes were *very* tightly clustered near the mean. The core (and thus the strong point, since in this situation the two are identical) predicted quite well in this game.

Our second example is the third of the three five-voter games used in Fiorina and Plott 1978; this one has voter ideal points at (51, 59), (30, 52), (25, 72), (62, 109), and (165, 32). This game is without a core. We show Copeland values for the points in this game in Figure 5.

Fiorina and Plott (1978) consider sixteen different solution concepts generating eight distinct predictions for game 3. For this game, which lacks a core, none of the solution concepts was of much value. The observed outcomes were rather far from any of the points predicted by any of the solution concepts.

Nonetheless, in this game, observed outcomes were still tightly clustered highly around a mean observed outcome of (45, 62), albeit not quite as tightly as in the games with a core (a standard devia-

**Figure 4. Approximate Distribution of Copeland Values in a Five-Voter Game with a Core, with Voter Ideal Points at (39, 68), (30, 52), (25, 72), (62, 109) and (165,32)**

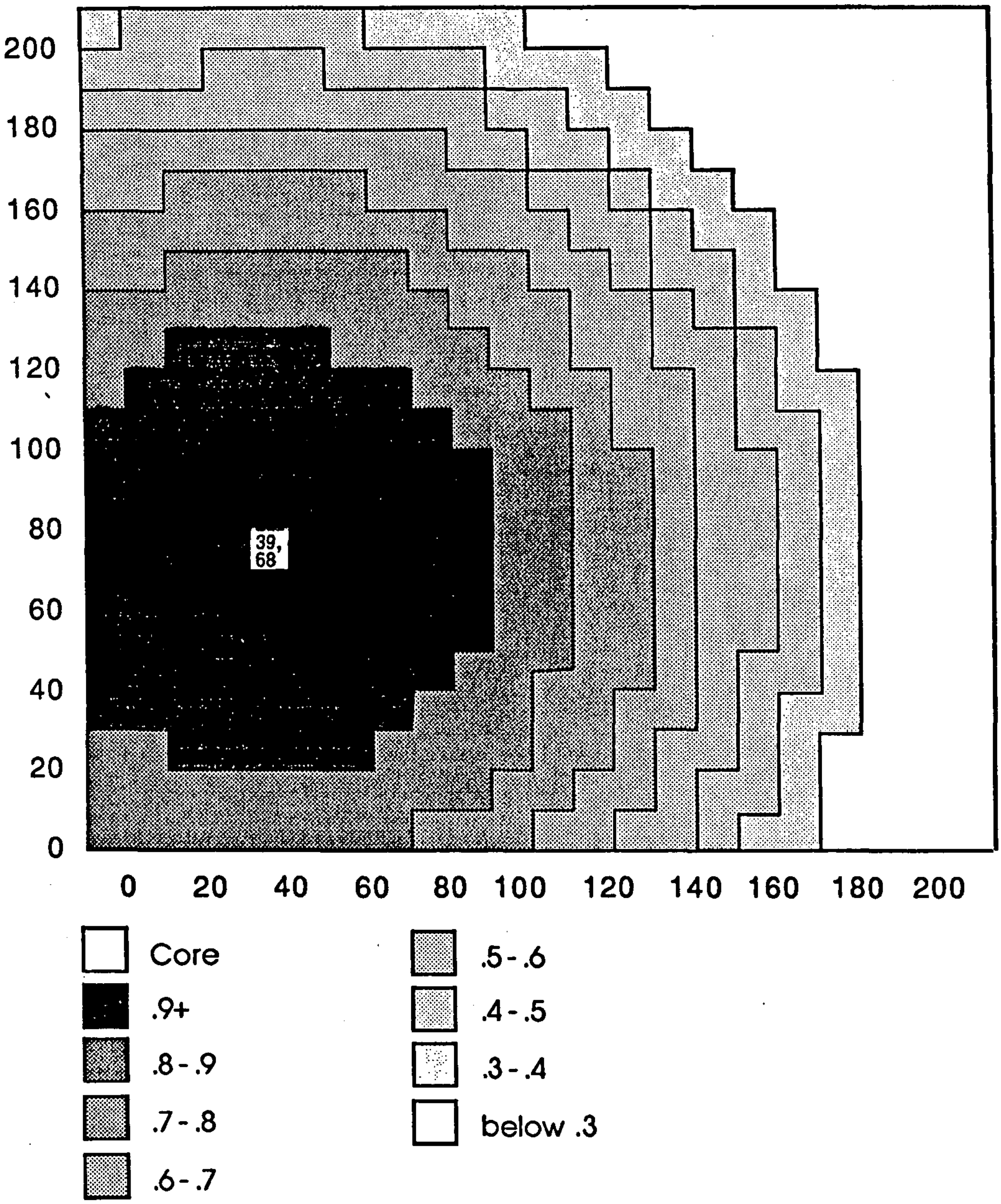
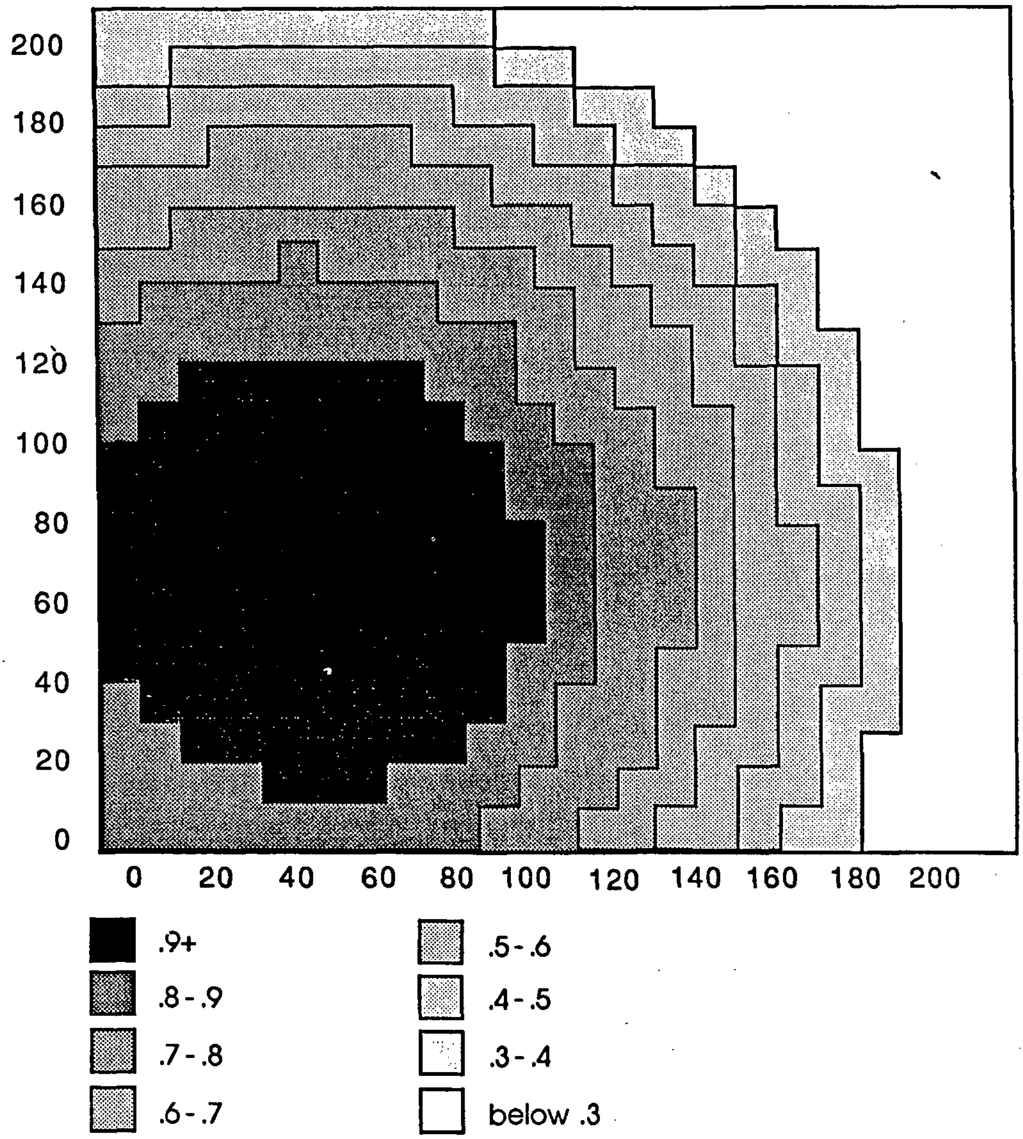


Figure 5. Approximate Distribution of Copeland Values in a Five-Voter Game without a Core, with Voter Ideal Points at (51, 59), (30, 52), (25, 72), (62, 109), and (165, 32)



tion of 10.3 in this game versus standard deviations of 5.2, 7.3, and 8.3 in various high-payoff experimental conditions in the two games with a core). Fiorina and Plott (1978, 590) contrast this finding of tightly clustered outcomes with the "chaos" suggested by McKelvey's (1976) result that "from any point in the space one can construct a sequence of alternative policies which under sincere voting lead to any arbitrarily selected point." They also note that they "did not notice any behavioral differences" and that "subjects in experiments on Game 3 appeared to have no greater difficulty in reaching a decision than did those in Game 1." Commenting about games without a core but whose outcomes were tightly clustered in a fashion inexplicable by any of the numerous theories they looked at, Fiorina and Plott (1978, 590) state "[We] wonder whether some unidentified theory is waiting to be discovered and used," and they go on to suggest that "if some as yet undeveloped theory is driving the . . . experiments (with game 3) it had better specialize to the . . . core when the latter exists."

We claim that the hypothesis that outcomes will be clustered at or near the strong point is just such a theory—reducing to the core when one exists and capable of accounting for observed outcomes in majority voting games without a core such as those in game 3 in Fiorina and Plott (1978). The strong point of game 3 is (47, 62), a value obtained from use of Equation 1, as solved earlier and confirmed by our simulation. It predicts the mean outcome in that game (45, 62) almost perfectly. The fit of the strong point in game 3 is as good as the fit of the core to the Fiorina and Plott (1978) experimental outcomes in the two games they used that had cores.

Of course, we are not claiming that the strong point can account for voting outcomes in all majority-rule games. Although the core is far and away the best single predictor of outcomes in games where a core exists (and the strong point reduces to the core when a core exists), it may not be chosen if the group focuses on ethical judgments related to equity norms (Eavey and Miller 1982). Also, it is well known that institutional features of choice (e.g., veto power, see Wilson and Herzberg 1984) or restrictions on feasible amendments such as a germaneness rule (Shepsle 1979) can significantly affect outcomes. In such cases we would need to modify our model.[5]

Often such modifications can be done simply. For example, one common rule for legislative decision making is to require a final vote against the status quo (Black 1958; Farquharson 1969).[6] For this rule, the modifications to Equation 1 needed to find the point with the highest Copeland value among the *feasible* outcomes (those in the win set of the status quo) are quite straightforward. We merely find the closest tangency of the family of circles defined in Equation 2 (itself, in turn, derived from the strong point defined by Equation 1) to the win set of the status quo; that is, we find the point in the win set of the status quo that is closest to the overall strong point in the space. The fact that Copeland values decline monotonically from the overall strong point provides the justification for this point of tangency's being the point in the win set of the status quo with the highest Copeland value. However, this point is not necessarily the one that beats the maximum number of points in the win set itself.

### Implication for Agenda Processes

Consider an open-agenda process in which alternatives are proposed with equal likelihood and that has some deterministic stop rule (e.g., stop if an alternative defeats *k* successive challengers). Then the likelihood that an outcome will be chosen is a function of its Copeland value; the probability density for the open-agenda process with such a stop rule will be even more thickly massed around the strong point than will the distribution of Copeland values because points that defeat few other points will be eliminated quickly. The open-agenda process with a *k*-defeats stop rule is a relatively unconstrained one. In more constrained (and more realistic) agenda models (e.g., one in which the likelihood that a point will be proposed is a function of the number of majority coalitions that prefer it to the status quo [Ferejohn, Fiorina and Packel 1980]), outcome probabilities will be even more sharply massed around the strong point than is true in the open-agenda process. The same is true, we believe, if we were to compare the open-agenda process to one in which proposed policies must be elements of the uncovered set (Shepsle and Weingast 1984, 65 n. 17), at least as long as there is some sort of stochastic stop rule. Thus, to establish that Copeland values fall off monotonically with distance from the strong point guarantees us that most reasonable agenda processes will, with very high probability, result in outcomes at or near the strong point.

If a known status quo will be in the final pairing, then, as noted above, the strong point can be defined relative to the set of alternatives that defeats the status quo. If the final vote always involves the previously defined status quo, then legislators have an incentive to propose alternatives with a high probability of beating those against which they might subsequently be paired. Again, the probabilities of choice will be highest for alternatives near the strong point because these are the ones whose "viability" is greatest.[7]

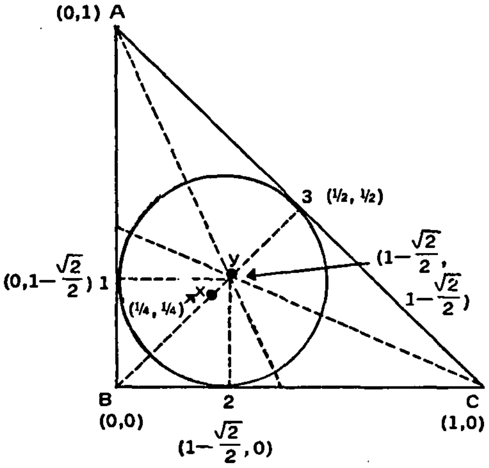
Even if legislators try to manipulate the agenda by introducing alternatives so as to eliminate other alternatives (a situation not true for the Fiorina and Plott games), under standard amendment procedure, as long as legislators cannot be certain that any given vote will be the final vote (or the next-to-final vote), then alternatives that defeat others are likely to be proposed and chosen.[8] Moreover, when voters are sophisticated, the outcomes of the usual legislative procedures will lie within the uncovered set (Shepsle and Weingast 1984), in which the strong point is a central point.

## The Strong Point and the Center of the Yolk

Our results are closely related to an important recent body of work whose full implications have not yet been absorbed into the established wisdom of social choice. A group of scholars (Ferejohn, Fiorina, and Packel 1980; Ferejohn, McKelvey, and Packel 1984; Hoffman and Packel 1981; Packel 1981) have shown that even in the absence of a core, certain plausible sincere majority-rule processes are well behaved in the sense that there exist stochastic limiting distributions that will characterize the set of expected outcomes and that these distributions can be expected (almost always) to confine outcomes to a relatively small section of the space. The area in which outcomes will cluster is centered around what they call the *yolk* of the voting game.[9]

It would be nice if the strong point corresponded to the center of the yolk. Unfortunately it does not, except when there is a core—although it does appear that the center of the yolk and the strong point are not very far away from each other in the examples we have looked at. In particular, in the Fiorina and Plott game without a core shown in Figure 5, the center of the yolk is approximately (46, 64). For that game we have previously found the strong point to be (47, 62). To aid the reader in understanding the connection between the center of the yolk and the strong point, we present a simple example. Consider the right triangle with ver-

**Figure 6. Comparison of the Location of the Center of the Yolk, *y*, with the Location of the Strong Point, *x*, in a Simple Three-Voter Game**



tices (0, 0), (0, 1), and (1, 0) shown in Figure 6.

Using Equation 1, the strong point of that triangle can be shown to be (1/4, 1/4). The *yolk* (Ferejohn, McKelvey, and Packel 1984) is the minimum sphere that intersects all median hyperplanes. In two-dimensional space with three voters, the median hyperplanes are simply the lines through the vertices of the triangle that pass within or through the edges of the triangle. Hence, in Figure 6, the center of the yolk is simply the center of the largest circle that can be inscribed within the triangle. Some simple geometry shows that this point is $(1 - \sqrt{2}/2, 1 - \sqrt{2}/2) = (.293, .293)$.

It seems apparent that with a more symmetric distribution of ideal points, the center of the yolk would be even closer to the strong point. If there is a core, of course, the two concepts are identical because the yolk collapses to a point, that is, collapses to the core (which is the strong point). Moreover, very preliminary work suggests that the strong point will also usually lie close to other recently proposed solution concepts.

## The Strong Point and the Modified Shapley Value

Owen (1971) introduced a generalization of the Shapley value based on coalitional probabilities that were a function of ideological proximity. Shapley (1977) modified the Owen (1971) model to provide a further nonsymmetric generalization of the Shapley value. These papers developed a new type of Shapley value applicable to voting games where plays are located in some Euclidean "ideological" space (see also Owen 1972; Straffin 1977).

THEOREM 2. *The strong point, $x$, can be expressed as*

$$x = \sum_{j=1}^{n} \phi_j P_j \qquad (3)$$

*where the $P_j$ are, as before, the locations of voter ideal points, and the $\phi_j$ are the modified Shapley values as defined in Shapley 1977.*

While the formula of Equation 3, like that of Equation 1, is quite simple, the proof of Theorem 2 (Shapley and Owen 1985) is very complex.

The usual Shapley value can best be explained by saying that voter *i*'s power is the probability that, in a randomly chosen ordering of the *n*-players (with all orderings having equal probability), voter *i* will be in pivot position, that is, the coalition of all members preceding *i* in the order loses, but it wins with *i*'s cooperation (Straffin 1980; Owen 1982). The modified Shapley values of both Owen and Shapley treat players as points in Euclidean space of some dimension; the power value is then defined as the probability that a given player will be pivotal in a randomly chosen ordering, the difference lying in the probability distributions that underlie this ordering. In Owen's scheme, a point in the space is chosen via a uniform distribution; orderings are taken in terms of the players' increasing distances from this randomly chosen point; while in Shapley's scheme, a direction in space (all points in the dual space) is chosen via a uniform distribution; orderings are then taken in terms of the players' locations along each direction.

To prove Theorem 2, Shapley and Owen (1985) make use of the fact that both the modified Shapley values and the Copeland values can be expressed in terms of $P_j(\Theta)$, where $P_j(\Theta)$ is the ideal point of the voter, $j$, who is pivotal when the ordering is in the $\Theta$ direction, that is, $j$ is the median voter for voter projections onto all lines that make an angle of $\Theta$ with the origin (which may be arbitrarily chosen). In particular, if we let $A(q)$ be the Copeland value of voter $q$, then

$$A(q) = 2 \int_0^{\pi} \{U(\Theta) \bullet [P_q(\Theta) - q]^2\} d\Theta \quad (4)$$

where $U(\Theta)$ = the unit vector in the direction of $\Theta = (\cos\Theta, \sin\Theta)$. Equation 4 gives us the area of the set of points that beats $q$.

On the other hand,

$$\phi_q = 1/\pi \int_0^{\pi} P_q(\Theta) d(\Theta),$$

that is, the power of a player (as defined by his or her modified Shapley value) is simply the measure of the set of angles for which he or she is pivotal.

## Conclusions

We believe that the strong point is a powerful solution concept that provides a plausible explanation for the stability and centrality of committee and legislative choices. The strong point is well defined and well behaved, easy to calculate, reduces to the core when there is a core, lies within the uncovered set, is responsive to individual differences in preferences, is centrally located in the Pareto set (thus guaranteeing that outcomes will not be too far from what most voters want), and explains hitherto inexplicable regularities observed in experimental settings. By specifying the Copeland winner in the spatial context and by showing its deep mathematical connection to the (modified) Shapley value, we also provide theoretically useful linkages between several distinct areas of positive political theory: social choice theory (in which the Copeland winner has long been known to have various desirable axiomatic properties), game theory, and spatial modelling.

As previously noted, it is our view that the search for a single ultimate solution concept for all political games is a futile one. There simply is no reason to believe that, say, coalitional models of situations involving politics among voting blocs will give the same answers as models based on, say, the institutional structure of two-candidate party-based competition.[10] The solution concept we have proposed, the Copeland winner in the spatial context, was reviewed in the context of legislative voting games in a pure majority-rule setting under standard amendment procedure. Its usefulness in other settings is a matter for further research, but we believe the strong point has already been shown to be a powerful solution concept in one important institutional setting, that of the legislature.[11] However, we would note that other recently proposed solution concepts such as the center of the yolk (Feld, Grofman, and Miller 1985; McKelvey 1986) are likely to lie near the strong point and thus that it may be quite difficult to determine which model offers the best fit to experimental results.

## Notes

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1. McKelvey (1986) and Moulin (1984) have shown that the Copeland winner must lie within the uncovered set. The uncovered set is the set of points that can defeat all other points on the space either directly or at one remove (Miller 1980). A wide variety of plausible sincere processes for agenda formation considered in Ferejohn, McKelvey, and Packel 1984, have been shown to lead voters to points in or near the uncovered set; while Miller (1980), McKelvey (1986) and Shepsle and Weingast (1984) have shown that sophisticated voting under standard amendment procedure leads to outcomes within the uncovered set. Our simulations have shown the strong point to be near the center of the uncovered set, at least for three-voter and five-voter games.

2. See n. 1.

3. We can state this result more precisely. Let the *yolk* be the minimum circle (sphere) that touches all median lines (hyperplanes), and let $r$ be the radius of the yolk. Then it is known that the uncovered set lies within $4r$ of the center of the yolk (McKelvey 1986) and may be even closer than that; and it is also known that the strong point is in the uncovered set (McKelvey 1986; Moulin 1984). Furthermore, it is known that no point $x$ that defeats some point $y$ can be more than $2r$ further from the center of the yolk than is $y$ itself (Feld, Grofman, and Miller 1985). Thus, even in the most extreme case, the only points that could defeat the strong point would lie within 6 radii of the center of the yolk. When the number of voters is large, a circle of radius $6r$ will be well within the Pareto set. We do not need to worry about points outside this central circle because they will not defeat the strong point and thus are irrelevant in defining the area of the strong point's win set. The $6r$ bound is, moreover, a very extreme one; for example, if the strong point were at the center of the yolk, then the only relevant points would be in a circle of radius $2r$. Because the strong point is likely to be close to the center of the yolk (see discussion later in the text), points within just slightly more than $2r$ of the yolk are the only ones with which we will need to concern ourselves in calculating the location of the strong point—the feasibility or infeasibility of other points is irrelevant. Thus, in absence of peculiar conditions that rule out the central region of the space as feasible, we may use Equation 1 to calculate the location of the strong point.

In experimental games (see, e.g., Fiorina and Plott 1978) and in the real world (where the only proposals to be taken seriously will be in some limited domain of the policy space), it is quite common for players to be restricted to alternatives from some specified domain of the space. It is also common for imposed utility schedules to be nonmonotonic with distance, for example, ellipsoidal or even kinked indifference curves.

In the Fiorina and Plott games, the imposed utility functions were not monotonic with distance and there were some important zones of discontinuity (payoff breaks). Margolis (1982, 114–98) argues that these discontinuities made the core points or points near them "prominent" outcomes because they fell in the set of "fair" alternatives defined by the domain in which all voters got some "reasonable" amount of utility. Thus, Margolis attributes corelike outcomes to processes quite far removed from purely self-interested calculations. He further argues that the protocols support such a "group-payoff" interpretation.

4. In our simulation, a delimited set of feasible points was used to calculate Copeland values. In finding the strong point analytically, we have measured area relative to a surface that—by the nature of the geometric construction—is confined either near to or within the convex hull (see Figure 1 in Shapley and Owen 1985). In our simulations, we permitted comparisons with points in a much larger area of the space. Clearly, as we enlarge the space of points with which comparisons are to be made, the absolute Copeland values will change. As we move away from the hull, the points exterior to the hull are largely Pareto-inferior to those at or in the hull; any point in the Pareto set will beat almost all the points in the space. Thus, how far out we go for our comparisons will affect the Copeland scores in absolute terms. If the convex hull is asymmetrically located with respect to the boundaries of the space whose points we are using for our comparison, extending the boundaries of the space may even marginally affect relative Copeland values and thus the location of the strong point, but, for reasons discussed in n. 3, adding points distant from the center of the yolk will not affect the win set of the strong point. Thus, if we are either "close in" enough or go far enough "out," such asymmetries will be irrelevant. In any case, the effects appear to be minor at worst.

If the yolk is small relative to the Pareto set, we conjecture that the exact shapes of voter indifference curves (e.g., circular vs. elliptical) is of minor importance in determining the strong point (Copeland winner). Even though the induced indifference curves in the experimental games studied by Fiorina and Plott (1978) are not circular, the values we have determined for the strong point in those games fit the observed outcomes in these games quite well.

5. More generally, we believe that processes of choice that involve committees or legislatures voting over sets of alternatives (see, e.g., Miller 1980; Miller, Grofman, and Feld 1985) may lead to different outcomes than those that involve choosing among competing coalitions or protocoalitions (see, e.g., Grofman 1982; McKelvey, Ordeshook, and Winer 1978) or between *two* competing political parties/candidates.

6. In standard amendment procedure (SAP, see Farquharson 1969; Grofman 1969), in order to win, an alternative (e.g., a main motion as amended) must also defeat the status quo.

7. Then, under SAP, the strong point will only be chosen if it is among those points that can defeat the status quo. Of course, if we treat the status quo as a random variable, then the strong point, since it defeats most points in the space, will also probably defeat the status quo.

8. We use the term *agenda* in the technical sense of a set of alternatives to a given motion to be voted on in a given sequence by a group such as a legislature (see, e.g., Farquharson 1969; Grofman 1969; Miller 1977), rather than in the broad sense of Cobb and Elder (1972) as the set of issue domains that are considered appropriate for legislative action.

9. See n. 3.

10. Cf. Ferejohn, Fiorina, and Weisberg (1978, 160-61). See also n. 5.

11. Glazer, Grofman, and Owen (1985) show that the strong point is a natural choice for an incumbent in two-candidate electoral competition if we assume that, due to electoral uncertainties, the challenger position can be thought of as a uniformly distributed random variable. Even if we do not make such an assumption, Glazer, Grofman, and Owen (1985) note that the strong point is a desirable point for candidates if there is a stochastic stop rule in an iterated sequence of candidate locational choices because the strong point beats most points in the space.

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## Forthcoming in September

The following articles, controversies, and research notes have been tentatively scheduled for publication in the September 1987 issue:

David Austen-Smith and William Riker. "Asymmetric Information and the Coherence of Legislation."

Francis A. Beer, Alice F. Healy, Grant P. Sinclair, and Lyle E. Bourne, Jr. "War Cues and Conflictual Foreign Policy Acts: A Laboratory Experiment."

Jonathan Bendor, Serge Taylor, and Roland van Gaalen. "Stacking the Deck: Bureaucratic Missions and the Search for Alternatives."

Steven J. Brams and D. Marc Kilgour. "Threat Escalation and Crisis Stability: A Game-Theoretic Analysis."

Robert S. Erikson, John P. McIver, and Gerald C. Wright. "Explorations in Political Geography: State Culture and Political Attitudes."

Michael R. Hawthorne and John E. Jackson. "The Individual Political Economy of Federal Tax Policy."

Gregory J. Kasza. "Bureaucratic Politics in Radical Military Regimes."

Keith Krehbiel, Kenneth A. Shepsle, and Barry Weingast. "Institutional Constraints on Congressional Committee Power." A Controversy.

John Langton and Mary G. Dietz. "Trapping or Teaching the Prince." A Controversy.

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Helmut Norpoth. "Guns and Butter and Government Popularity in Britain." A Research Note.

Robert Powell. "Crisis Bargaining, Escalation, and MAD."

Lawrence A. Scaff. "Fleeing the Iron Cage: Politics and Culture in the Thought of Max Weber."

Laura L. Vertz, John P. Frendreis, and James L. Gibson. "Nationalization of the American Electorate: A Multi-Office Perspective." A Research Note.

## Erratum

Bruce Bueno de Mesquita and David Lalman, "Reason and War" (December 1986, 1113–29): On page 1116, column 1, the first equation should read

$$P^i(Esc_i) = (E^i(U_{ij})+3)/6$$

# CONTROVERSY

# REBELLIOUS COLLECTIVE ACTION REVISITED

*Why does it happen that ordinary people can come to participate in rebellious collective action? In the June 1986 issue of this* Review, *Edward N. Muller and Karl-Dieter Opp argued a public-goods model to account for why rational citizens may become rebels. They offered empirical data drawn from samples in New York City and Hamburg, Germany in support of the public-goods model. George Kolsko takes issue with the rationale of Muller and Opp, arguing that their public-goods model is not a rational-choice explanation of rebellious collective action. In response, Muller and Opp clarify their theory and further elaborate its assumptions.*

Though Muller and Opp locate their attempt to explain rebellious collective action within rational-choice analysis (Muller and Opp 1986),[1] I believe that there are problems with this assessment. These will become clear if we rough out some basic criteria of what constitutes a rational-choice explanation. Muller and Opp are not explicit about these matters, but I imagine that they would agree with the majority of scholars and accept the assumptions that rational-choice models explain social behavior as the behavior of individuals whose actions are (1) self-interested and (2) rational, that is, efficient or maximizing.

Assumption 1 is straightforward and need not detain us. Rational-choice theory makes the intuitively plausible assumption that individuals generally act in the pursuit of their own welfare rather than the welfare of other people, especially others who are not close friends or family members. The conception of an individual's welfare posited by rational-choice theory is what individuals generally regard as central to their well-being, most frequently such goods as economic rewards, power, pleasure, and reputation.[2] As for Assumption 2, theorists in general define *rationality* in reference to the application of suitable means to the attainment of given ends. With his usual lucidity, Rawls describes *rationality* as follows:

> The concept of rationality invoked here, with the exception of one essential feature, is the standard one familiar in social theory. Thus in the usual way, a rational person is thought to have a coherent set of preferences between the options open to him. He ranks these options according to how well they further his purposes; he follows the plan that will satisfy more of his desire rather than less, and which has the greater chance of being successfully executed. (1971, 143)

Rational-choice theorists hold that rational individuals are good judges of their own interest and pursue it efficiently. Faced with a number of different possible courses of action, *A* is assumed to be able generally to identify and to pursue that which is most conducive to personal welfare.

Because we will be treading on slippery terminological ground, some clarification is in order. The term *rationality* (and cognate words) are used in different senses by different theorists. The sense under dis-

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cussion here is that prevalent in rational-choice analysis, the main thrust of which is efficient maximization of values, as has just been seen. Though Muller and Opp do not make this point explicitly, this is obviously the sense that they have in mind. In order to be as clear as possible, I will place the word *rationality* (and cognate words) in quotation marks when a wider construal is possible.

Returning to Muller and Opp, we can see that their analysis departs from Assumption 1. One of their major purposes is to argue against what they call a "private interest" account of rebellious collective action, in favor of a "public-goods model." The difficulties with the private-interest model are well-known (see, esp., Olson 1971). As long as the community is sufficiently large, it is highly unlikely that *A*'s participation in some protest activity will affect the likelihood of the movement's aims being realized. Since *A* is unable to influence the provision of the goods in question, their value should not be a factor in motivating *A* to participate. If we assume that some costs would be incurred by participation, it is not rational for *A* to take part. In order to account for the fact that individuals do protest, various additions to the private-interest model have been suggested, especially regarding "selective incentives" (Olson's term [1971, 51, 135–36]), personal benefits that individuals gain only by participating. For instance, Tullock (1971) makes much of the entertainment value of participating in various political activities.

Muller and Opp's public-goods model rests upon the hypothesis that individuals might be willing to forsake individual rationality for group rationality (pp. 473–74, 480). In addition to calculating their own ability to contribute to the provision of desired public goods, individuals might take into account the effectiveness of the group as a whole (pp. 473–74). Because of the greater effectiveness of groups, it follows from the public-goods model that individuals can rationally choose to participate, provided that they also have the requisite degree of concern for the public goods. Thus, unlike the private-interest model, the public-goods model does predict an association between rebellious collective action and the value of the public goods it is intended to realize, a conclusion that is supported by Muller and Opp's survey data.

Muller and Opp's postulation of collectively rational individuals who are concerned with the provision of public goods entails some modification of Assumption 1. This appears to be one factor in the public-goods model's "more precise specification of the nature of psychological incentives" (484). But we need not dwell on the complexities here. More important for our concerns is that Muller and Opp do not appear to realize the extent to which they modify Assumption 2 as well. There can be no doubt that they *believe* that the subjects they discuss are rational actors (see pp. 478, 480, 484). Though they do not make clear exactly what they mean by this, it is apparent that they view them as acting in accordance with Assumption 2, as efficient maximizers of the group's interest. It is upon this claim that I will concentrate.

As noted above, a basic assumption of rational-choice models is that, by participating in rebellious collective action, the individual will have a negligible effect upon the realization of the goods he or she pursues. Thus *A*, who seeks to further the group's interest is placed in the difficult position of having no means realistically to do so. The question, then, is what is the rational thing for *A* to do if unable effectively to advance this end?

According to one possible response to this question (Response 1), it is rational for *A* to participate in rebellious collective action. Response 1 depends upon distinguishing effective action from efficient action. To be rational an action must fall under the latter category but not necessarily under the former. By taking part in rebellious collective action, *A* will be advancing the end *as far as he or she can.* Because rational action is the selection of the most suitable means for one's ends under given circumstances, *A*'s behavior in Response 1 should be viewed as rational. Though *A*'s participation has only negligible effect, negligible effect is better than none.

There are difficulties with Response 1. Participation in rebellious collective action can be costly to *A*. But according to the public-goods model, collectively rational individuals are willing to subordinate their own interests to the interests of the group. As is indicated by Muller and Opp's curious finding concerning the incentive effects of the expected negative consequences of participation (which Muller and Opp dub a " 'martyr' syndrome" [pp. 483, 485]), individuals are perhaps willing to disregard their own interests to a high degree. Despite the possible costs of action, then, *A*'s participation should still be viewed as rational.

By arguing along the lines of Response 1, Muller and Opp would be able to hold that the behavior of their subjects is both group-interested and rational. This would seem to be in accordance with their apparent belief that their public-goods model entails modification of Assumption 1 but not Assumption 2. However, Muller and Opp do not respond in this way. According to the public-goods model (Response 2), *A*'s participation is rational because *A* *believes* that it matters (pp. 478, 484). One of Muller and Opp's more interesting findings is that their subjects miscalculate the probable effects of their participation. They believe that their actions have "considerable personal influence on the provision of public goods" (p. 478). Hence Muller and Opp build into their conception of collective rationality a connection between *A*'s view of the effectiveness of his or her own participation and of the effectiveness of the group (pp. 478, 480, 484). However, the crucial point that they overlook is that if *A* acts on the basis of such an egregious miscalculation, *A* no longer efficiently pursues his or her goals. Because *A*'s behavior is at variance with Assumption 2, Response 2 is unable to preserve the rationality of participation in rebellious collective action.

Because "rationality" is a complex concept, Muller and Opp have a possible reply to this objection. They could argue that rational actors efficiently pursue their goals *in accordance with their beliefs* concerning the circumstances that they face. Theorists who discuss "rationality" generally make implicit reference to the subject's beliefs about the circumstances.[3] This is made explicit by Elster: "That [an] action is rational means that given the beliefs of the agent, the action was the best way for him to realize his plans or desires." (1985a, 9). If Muller and Opp construe "rationality" in this sense, they can say that, because *A* believes that participation will be efficacious, it is therefore rational to participate.

I do not believe that this reply gets Muller and Opp off the hook. It is clear that there are constraints upon the beliefs in accordance with which an agent can act and still be considered rational. In a given choice situation, the rational agent will choose the greater of two possible goods. Thus, faced with two possible courses of action—*X* promising to yield 10 units of goods and *Y* promising to yield 20—the rational agent will choose *Y*. An agent who chooses *X* under the belief that 10 is greater than 20 should surely not be viewed as rational.

A familiar distinction can be introduced to clarify matters here. In the *Ethics*, in discussing the mitigating effects of ignorance upon moral culpability, Aristotle distinguishes ignorance of general rules from ignorance of particular circumstances (see, esp., *Nicomachean Ethics*, book 3, chap. 1). The details of Aristotle's account need not concern us, nor must we develop this distinction with great precision. Basically, I believe that rational-choice analysis can profitably employ a wider conception of "rationality" in certain cases. I believe that *A* can still be viewed as rational if acting out of mistaken beliefs concerning the specific facts of the case. But if acting according to mistaken general principles, especially simple and important principles that individuals can ordinarily be expected to know, then *A* should be viewed as forfeiting a claim to rationality. If *A* is hungry and believes that the wooden apple on the table is real, a decision to eat it can be viewed as rational, especially if the wooden apple is skillfully made and looks real. But if *A* is hungry and knows that it is a wooden apple, a decision to eat it is not rational, even if *A* believes that wooden apples are delicious and nourishing. Rational people do not hold such beliefs.

Along similar lines, individuals who participate in rebellious collective action in large communities, under the assumption that their own participation will appreciably affect the likelihood of their goals' being realized, should not (regardless of whether they are self-interested) be viewed as rational actors. The mistake that they make concerns one of the fundamental principles of rational-choice theory. It is a mistake that no rational person should make.[4] I conclude, then, that Muller and Opp's public-goods model departs from Assumption 2 in addition to Assumption 1 and so should not be viewed as falling within the family of rational-choice explanations.

The above criticisms of Muller and Opp concern more than a question of semantics. In all probability, Muller and Opp locate their public-goods model within rational-choice theory because of the great strengths of such analysis. Very briefly, rational-choice theory allows the construction of simple but powerful, testable explanations. To use Barry's words, the advantage of the method is that "in appropriate kinds of situations, it enables us, operating with simple premises concerning rational behaviour, to deduce by logic and mathematics interesting conclusions about what will happen" (Barry 1978, 15–16; and p. 5 n. 2). If we are able to determine the relative values of the goods that will be realized by the pursuit of different possible courses of action, we will be able to predict how individuals will behave. But we will be able to do so only if individuals behave in accordance with Assumptions 1 and 2 (especially 2). In departing from conventional rational-choice analysis, Muller and Opp threaten the elegance and simplicity of the model. That their subjects do not understand the most basic facts about the likely consequences of their actions makes their actions much more difficult to predict and explain. In addition to having to determine the values attached to different courses of action, the researcher must deal with an additional set of variables concerning how individuals will regard these options. Olson notes that rational-choice explanations can be distinguished from other explanations that stem from psychology or social psychology (1971, 161–62; also Simon 1985). Though Muller and Opp apparently do not realize this, their public-goods model—with its "more precise specification of the nature of psychological incentives" (p. 484)—falls into the latter camp rather than the former, while explanations of this kind forfeit much of the lucidity of the rational-choice model.

My remarks here do little to undermine the importance of Muller and Opp's empirical findings. Even if the public-goods model should not be viewed as a rational-choice explanation, to the extent that it helps to explain participation in rebellious collective action, it will, of course, make an important contribution. But, in at least one respect, the contribution seems to me to be different from what Muller and Opp have in mind. The fact that, according to the public-goods model (as revised here), individuals' decisions to participate in collective action are neither (1) self-interested nor (2) efficient indicates something of the limits of rational-choice analysis. As Muller and Opp's results suggest and as seems apparent to me, one reason that rational-choice theory has had trouble explaining a wide variety of political behavior is that individuals engage in such activity for a wide variety of "nonrational" reasons. Of course, there is nothing to prevent rational-choice theorists from reinterpreting such motives and subsuming them under the all-purpose category of "selective incentives," though the cost of doing so is, again, to deprive rational-choice theory of its simplicity and elegance. As scholars have argued, and as is surely suggested by the " 'martyr' syndrome" that Muller and Opp observe, among the important factors motivating individuals to take part in political activity are concerns of a moral nature, which rational-choice theory promises to have no easy time accommodating (see, e.g., Benn 1979; Sorel 1950).

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Our 1986 paper in the *Review* was concerned with an application of the rational-choice model (RCM) to explain rebellious collective action. It was not possible there to spell out in detail the version of the RCM on which our explanation of rebellious collective action was based. We therefore welcome the comments by Klosko, which provide us the opportunity to clarify and elaborate on the theoretical foundation of our research.

Any rational-choice explanation of behavior must include, according to Klosko, two assumptions: (1) individuals are *self-interested*, and (2) their behavior is *efficient*. The question is, Are these assumptions consistent with those of our version of the RCM?

The version of the RCM that underlies our explanation of rebellious collective action includes the following assumptions:

1. *The preference assumption.* Individual preferences are determinants of actions that are—in the perception of the individual—instrumental in satisfying the respective preferences.
2. *The constraint assumption.* Constraints/opportunities imposed on/available to individuals are determinants of their actions.
3. *The utility maximization assumption.* Individuals choose to perform those actions that will realize their preferences to the greatest extent, taking into account the constraints/opportunities imposed on/available to them.

The preference assumption and the utility maximization assumption clearly imply that individuals will seek to satisfy their own interests and are therefore self-interested. We do not introduce any restrictions on the kind of preferences that may be included in a rational-choice explanation of rebellious collective action (or of any other kind of behavior). In particular, the preference assumption does not imply that the interests of individuals are purely egoistic in the sense of being limited to their own private welfare. The exact nature of preferences is left open: they may include public goods, which will benefit all members of a group or broader society, including the individual; and they may include the private welfare of others (altruistic goals of collective action, e.g., the Sanctuary Movement). Because Klosko acknowledges in note 2 that the RCM "is compatible with a wider conception of the individual's interest," it is inconsistent for him then to claim that our public-goods model "should not be viewed as falling within the family of rational-choice explanations" because it departs from Assumption 1—as well as from Assumption 2. The public-goods model does not depart from Assumption 1, except under a quite narrow definition of self-interest, which Klosko himself concedes is arbitrary.

Klosko recognizes that our version of the RCM is compatible with Elster's (1985a) definition of rational action, "That [an] action is rational means that given the beliefs of the agent, the action was the best way for him to realize his plans or desires" (p. 9). In emphasizing the "beliefs of the agent," this definition allows for subjective misperception of individual or group influence on the satisfaction of preferences and therefore does not depart from Assumption 2, the efficiency assumption, even if the effectiveness of the individual and/or the group is calculated incorrectly according to an objective criterion. Thus, our version of the RCM implies that actors behave rationally according to Elster's definition (also see Elster 1985b)—indeed, when we spoke of rationality in our paper, we had this definition of rational action in mind. Klosko, however, does not think that a definition like Elster's "gets [us] off the hook." This must be because he would not include Elster's definition within the rational-choice "family."

The real issue for Klosko is not the question of how self-interest is defined in Assumption 1 but rather how *efficiency* is defined in Assumption 2. In this context, he refers to "the application of suitable means to the attainment of given ends." Whether an actor acts efficiently or not is thus judged by an outside observer, not by the actor. This is apparent when Klosko writes that if an actor miscalculates his influence on the provision of a public good, the actor "no longer efficiently pursues his or her goals," that is, does not act efficiently.

All average citizens acting in nonsmall groups miscalculate their influence if they think that their participation makes any difference to the outcome of collective behavior. Yet, in all the studies of political efficacy that have even been conducted, substantial numbers of individuals actually have reported the belief that their participation matters. Moreover, sense of political efficacy has been found consistently to correlate positively with level of political information and formal education. Thus, miscalculation of individual influence cannot be assumed to be just an aberration due to lack of knowledge about politics or inadequate education; it is typical of individuals who possess other attributes that one would expect to enhance the likelihood of rational decision making. Why?

We propose that many individuals who participate in rebellious collective action because of a high sense of efficacy (in part) do so not because they have a mistaken understanding of general principles but rather because they substitute a collectively rational general principle for the individually rational general principle of the conventional RCM, which is based on simple probability theory. The collectively rational general principle is that if all average citizens are individually rational, then nobody but the leaders of the movement will participate, and collective action will fail; consequently, an individual act of participation cannot be regarded as separable from that of the whole group. In other words, the collectively rational general principal is that the whole is greater than the sum of its parts.

Perceived influence, in the general RCM, operates as a constraint on preferences. Neither Elster's nor our definition of rationality stipulates that such constraints must conform to objective criteria of probabilistic logic, according to which the whole cannot be greater than the sum of its parts. As the violation of Assumption 1 depends on how one defines the term *self interest*, so the extent to which a rational-choice model is judged to depart from Assumption 2 depends on one's definition of the general principles by which efficiency is to be assessed.

The force of Klosko's critique of our version of the RCM thus depends on *which* definitions of self-interest and principles of efficiency are allowed into the family of rational-choice models. Definitions are, of course, not true or false but more or less useful. We reject the "strict" (or, as we would say, "narrow") definition of rationality preferred by Klosko and by many economists because we do not consider it to be useful for explaining rebellious collective action.

In proposing what we consider to be a more useful version of the RCM, we allow individuals to perceive their own personal influence on provision of public goods as being nonnegligible, and we introduce the likelihood of success of the group as determined by the past success of groups—that is, contingencies of vicarious reinforcement—as a relevant constraint on individual preferences. We assume—but without testing the assumption—that both personal influence and group influence can be determinants of the choice to participate in rebellious collective action, given public-good preferences, because individuals recognize that the success of rebellious collective action depends on the group's acting together as a whole. In effect, we allow individuals faced with a choice to participate or not in rebellious collective action to reject free-riding as being collectively irrational. Is this acceptable?

Economists are reluctant to accept a version of the RCM that includes a broad variety of motivations and constraints. One of the reasons mentioned by Klosko is that a broad model threatens "to deprive rational choice theory of its simplicity and elegance." Elegance and simplicity, however, are not the primary criteria to assess theories. A nonelegant and complex theory that is correct and has explanatory power will be preferred to one that is simple and elegant but is incorrect and lacks explanatory power.

The conventional economic RCM is widely recognized to lack explanatory power with respect to behavior such as rebellious collective action. We argue that its weak explanatory power is due to restrictive assumptions, which can be modified by introducing broader definitions of *self-interest* and (particularly) of *efficiency*, to use Klosko's terminology. The best way to assess the fruitfulness of our broad version of the RCM is to derive hypotheses from it that can be tested empirically. This was the main thrust of our research. We found that our "public-goods" model did have some nontrivial explanatory power in regard to rebellious collective action. In particular, we found support for the hypothesis that participation in rebellious collective action is a function of the product of public-goods preferences and perceived individual and group influence on provision of public goods. We did not actually test the assumption that perceived individual and group influence are both relevant *because* of a belief in the general principle that the success of rebellious collective action depends on all members of the group "doing their part"; but we found that individuals seemed to behave as if they accepted this principle. Therefore, our modification of the efficiency assumption to allow for what we call "collective rationality" appears to be useful.

One important priority of future research is to investigate empirically the question of whether individuals are unaware of or misunderstand the principle that their own personal participation will have a negligible effect on the realization of their public-goods preferences or whether they consciously reject this principle in favor of an alternative general rule that the success of collective action depends on the group's acting together as a unified whole. If the latter is the case, then our version of the RCM could be said to modify rather than depart from Assumption 2. If the former is the case, then we would have to acknowledge that the public-goods model does not conform to the efficiency assumption, but we still would not regard this as justification for dismissing it altogether from the rational-choice family. In any event, a second important priority of future research is to compare the explanatory power of our version of the RCM with that of other competing psychological and sociological theories and hypotheses about determinants of rebellious collective action. It may thus turn out that, although the RCM is burdened with problems, the problems of alternative theories are even more severe.

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## Notes

George Klosko expresses his gratitude to Steven Finkel for valuable discussions and comments on previous drafts.

1. Unaccompanied page references are to this article.

2. Although, as Muller and Opp indicate, rational-choice theory generally posits self-interested individuals (Bonner 1986, 6), the model is compatible with a wider conception of the individual's interest. It can accommodate individuals who pursue the welfare of other people in addition to or instead of their own. See, e.g., Barry 1978, 11–21; Buchanan and Tullock 1962, 18; Olson 1971, 65. Of the two assumptions, then, Assumption 2 is probably more important than Assumption 1.

3. For some of the complexities involved in assessing "rationality" in particular cases, see Elster 1983, chap. 1; Simon 1985.

4. Barry (1978, 18 n. 5) makes a very similar point (criticizing William Riker) in a work cited by Muller and Opp.

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# RESEARCH NOTES

# MEASURING THE SOUTHERN CONTRIBUTION TO THE DEMOCRATIC COALITION

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*Measuring the southern contribution to the Democratic coalition is an important task. To measure this contribution one must choose appropriate data and the appropriate unit of analysis for studying party coalitions in the United States. Two recent studies of party coalitions use the National Election Studies to estimate the southern contribution to the Democratic party, and these studies illustrate the problems one may encounter. This note demonstrates two points. First, survey research results may lead to erroneous estimates and it is preferable, where possible, to rely upon official election statistics. Second, the contribution of demographic groups to party coalitions should be assessed within the context of the political rules that make such coalitions meaningful.*

Between the end of Reconstruction and the end of World War II, the South was a Democratic stronghold. Since the war, the South has become a highly competitive region. The end of Democratic dominance in the South severely weakened the New Deal coalition, and students of partisan change in the postwar United States must analyze the transformation of this region.

Measuring the southern contribution to the Democratic coalition is an important task. To measure this contribution one must choose appropriate data and the appropriate unit of analysis for studying party coalitions in the United States. Two recent studies, one by Axelrod (1986) and one by Stanley, Bianco, and Niemi (1986), measure political change in the postwar South. These are both important studies that contribute to our understanding of the changing social composition of the Democratic party coalition. Axelrod provides a valuable service by updating his original analyses (Axelrod 1972) that lucidly spelled out the logic of party coalitions. Stanley, Bianco, and Niemi enrich our understanding by presenting a multivariate analysis that assesses the independent contribution that group memberships make toward Democratic party loyalties after controls are introduced for membership in other pro-Democratic groups. But both studies illustrate the problems that one can encounter in studying the southern contribution to the Democratic coalition.

Both Axelrod and Stanley, Bianco, and Niemi rely upon reanalyses of the National Election Studies (NES) conducted by the University of Michigan Survey Research Center from 1952 through 1984. Both include southerners as a basic component of the Democratic coalition, although they differ in their definitions. Axelrod includes all the residents of the southern and border states who vote for the Democratic presidential candidate,

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whereas Stanley, Bianco, and Niemi include only native white southerners in the 11 states of the old Confederacy who identify with the Democratic party. Both Axelrod and Stanley, Bianco, and Niemi shed light on the way the South has deserted the Democrats. But two questions can be raised. First, should political scientists rely upon survey research data when the election returns themselves provide better data? Second, is it politically meaningful to study the South as a demographic component of the Democratic coalition?

The first question applies only to Axelrod's analysis. If one wants to know the overall percentage of the Democratic presidential vote coming from the South (or the overall percentage of Republican votes coming from outside the South) one need not use survey data at all. More reliable results are available simply by adding up the Democratic votes for president in the southern and border states and dividing this sum by the total number of votes received by the Democratic presidential candidate. Such a procedure avoids the possibility of sampling error and does not require the iteration procedure that Axelrod employed (at least through 1976) to estimate the contribution of social groups to the respective presidential coalitions.

In 1960 and 1980 the results of such simple arithmetic differ substantially from those Axelrod reports. According to Axelrod's estimates, 27% of Kennedy's total vote came from the South (including the border states). In fact, the state-by-state election results show that 7,078,923 of Kennedy's 34,221,344 votes came from the South,[1] and thus the actual southern contribution to the Democratic presidential coalition was only 21%. The NES results suggest that over nine million Democratic votes came from the South. This error appears to result from two basic factors. The southern states actually made up only 29% of the voting-age population, whereas the NES data suggest that the South made up 34%. Moreover, turnout in the South was only 45%, whereas Axelrod estimated southern turnout to be 50%.[2]

According to Axelrod's estimates, 39% of Carter's vote in 1980 came from the South. The election results show that 11,448,624 of Carter's 35,483,820 votes came from the South, and thus the actual southern contribution to Carter's coalition was only 32%. The NES results suggest that nearly 14 million Democratic votes came from the South. It is hard to specify the source of error in this case because the three basic components of Axelrod's formula—the size of a group, its turnout, and its loyalty to the party—are all close to the actual result.[3]

In 1984, as Axelrod notes, the NES data show the South to be more Democratic than the nation as a whole, when in fact it was less Democratic. According to Axelrod's estimates, 32% of Mondale's total vote came from the South; in fact, only 29% did. Although an error of 3 percentage points seems acceptable by survey research standards, Axelrod is overestimating the southern contribution to Mondale by more than one million votes.

Errors of this magnitude or greater are found in three other elections. Axelrod's 1968 estimate of the southern contribution to the Democratic presidential coalition is four points greater than the actual contribution, and his 1972 estimate is five points too high. In 1952, on the other hand, Axelrod's estimate is three points lower than the actual southern contribution to Stevenson's total vote. In both 1968 and 1972, the primary source of error appears to result from overestimating the percentage of southerners who voted Democratic. Humphrey received only 34% of the southern vote, whereas Axelrod reports that southern loyalty to the Democratic party was 39%. McGovern won only 30% of the southern vote, whereas Axelrod reports that southern loyalty was 36%. Axelrod also overestimates Democratic success in the South in 1952. Stevenson won 51% of the southern vote, whereas Axelrod reports that southern loyalty was 55%. This error, however, was more than counterbalanced by substantially underestimating southern turnout. Census Bureau results show that southern turnout was 44%, whereas Axelrod reported southern turnout to be 35%.[4]

Only in 1956, 1964, and 1976 did Axelrod's estimates come within a percentage point of the actual southern contribution. Of course, we are normally forced to accept both substantial sampling and response error when we rely upon surveys. But there is no need to accept such error when far more accurate results are available.[5]

Stanley and his colleagues were forced to use survey results because the election returns cannot be used to measure the behavior of native white southerners. Moreover, Stanley and his colleagues were studying party identification, not actual votes. Stanley, Bianco, and Niemi provide a useful service in helping to identify the shift of native white southerners from the Democratic party.

But when they calculate the total percentage of Democratic party identifiers who are native white southerners (1986, tbl. 3), their results are not particularly useful. For while it is important to understand the group dynamics of party support, those dynamics must be understood in the political context in which elections occur. A fundamental characteristic of all southern votes (and of all the votes of native white southerners) is that they are cast in the South.[6] Because presidential elections are won by garnering electoral votes, the overall share of the popular vote received from the South (or from native white southerners) is of relatively little importance. A comparison of the 1960 and 1984 elections illustrates this point. In 1960, only 15% of Kennedy's total popular vote came from the 11 states of the old Confederacy. By 1984, the relative size of the South had grown and southern turnout was much higher. Twenty-three percent of Mondale's total vote came from these 11 states. But there is a crucial difference between these elections. In 1960, Kennedy won 81 of the South's 128 electoral votes, and the South thus provided over a fourth of Kennedy's 303 electoral votes. In 1984, the South yielded 138 electoral votes, and Mondale won none of them.

A simple examination of where the *electoral* votes come from shows that in three of the four postwar Democratic presidential victories (1948, 1960, and 1976) a substantial share of the total Democratic electoral votes came from the South and that all three of these victories required southern electoral votes. Of the six postwar Republican victories, only Nixon's 1968 win depended upon southern electoral votes.

This note should not detract from the fundamental importance of the Axelrod and Stanley, Bianco, and Niemi analyses. None of the other social groups studied by Axelrod and by Stanley, Bianco, and Niemi can be clearly defined as yielding a specific set of electoral votes. Admittedly, the residents of the central cities of the 12 largest metropolitan areas (an Axelrod category) live in specific states, and about half of all U.S. Jews (a Stanley, Bianco, and Niemi category) live in New York and California. But when groups are geographically dispersed it is perfectly reasonable to think in terms of their total contribution to a national coalition. The study of the South in these terms is questionable, however. For although the study of coalitions is essential to understanding postwar political change, coalitions should be studied in light of the political rules that make such coalitions meaningful.

## Notes

I am grateful to Cleo H. Cherryholmes, Ada W. Finifter, Thomas H. Hammond, Jeffrey S. Hill, Joseph A. Schlesinger, and Brian D. Silver for comments on an earlier version of this note.

1. My data source for the state-by-state results was Congressional Quarterly 1985. To maximize comparability with Axelrod, I used the NES definition of southern and border states. For these comparisons, therefore, I included the 11 states of the old Confederacy, plus Kentucky, Maryland, Oklahoma, and West Virginia. From 1964 on, the District of Columbia is included with the border states.

2. My estimate of the size of the voting-age population in the South as well as southern turnout are based upon state-by-state results reported in U.S. Bureau of the Census 1965. Of course, the NES surveys always reveal substantially higher levels of reported turnout than the actual result, but Axelrod employs an iteration procedure to correct for this bias.

3. Axelrod did not actually use his basic formula to compute the percentage contribution of social groups to the party coalitions in 1980 and 1984 (see Axelrod 1986, 284, n. 1). If his formula were applied, the southern contribution to the Democratic coalition would be 36%.

4. My estimate of southern turnout was based upon state-by-state results reported in U.S. Bureau of the Census 1956. This result is based upon the voting-age civilian population and probably overestimates turnout by about one percentage point.

5. Of course, the election returns also include some error. There may be some cases when survey data are more accurate than official voting statistics. Almond and Verba (1963, 292), for example, suggest that their survey results from Mexico may be more accurate in measuring party support than the Mexican voting statistics. But it seems realistic to assume that U.S. election returns are considerably more reliable in estimating the contribution of a region to a party's total vote than results based upon surveys.

6. Although the following discussion refers to the Democratic coalition in presidential elections, the same basic logic applies to Democratic representation in Congress. Studying the overall percentage of Democratic votes for congressional candidates in the nation as a whole that are cast in the South (or the overall percentage of Democratic party identifiers in the nation as a whole who are native white southerners) would not help a great deal in understanding the increasing share of southern House and Senate seats that are won by the Republican party.

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# POLITICAL SELF-EFFICACY TESTED

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*A subset of citizens in a democratic system directly test their political self-efficacy by petitioning government for assistance of one kind or another. Drawing on survey data gathered in India in 1967, this investigation focuses on the consequences of success or failure for perceived self-efficacy and for perceived government responsiveness. The analysis demonstrates that (1) successful petitioners come to enjoy a somewhat enhanced sense of self-efficacy but do not view government as particularly responsive, (2) unsuccessful petitioners do not see themselves as inefficacious but—possibly instead—do see government responsiveness in distinctly negative terms, and, (3) the kind of evidence that can help sustain a positive sense of self-efficacy will not suffice to undergird a belief in system responsiveness.*

Interest in the connection between perceived self-efficacy and behavior has produced a large and varied empirical research literature. Until recently, however, it was a literature of bits and pieces (no surprise, given the different research methods and disciplinary interests of its contributors), lacking a general theory to integrate the disparate research strands and give broader meaning to their findings. This void has been addressed by Albert Bandura (1977, 1982), who has organized a great deal of evidence from both laboratory and field studies (including a substantial amount emerging from his own research program), within a powerful theory of human coping behavior that places self-efficacy at its core. The essence of the theory is this: self-referent thought in general and self-efficacy judgements in particular mediate the relationship between knowledge and action, affecting both motivation and behavior. Within any particular domain of activity, feelings of self-efficacy strongly influence all coping efforts as well as diverse other phenomena, including physiological stress reactions, thought patterns, and emotional responses. People who perceive themselves to be efficacious resolutely confront environmental demands, whether emanating from private worlds or public. In so doing, they allocate attention and effort with relative efficiency, and they are not easily discouraged by obstacles.

Perceived self-efficacy, the theory continues, is a product of social learning. Four sources of information are important to this process: one's own performance attainments, vicarious experience of others' performances, verbal persuasion, and one's own physiological states. The first is the most influential of the four, but those that "operate through nonperformance modes are of particular interest because they provide no behavioral information for judging changes in one's self-efficacy. Persons have to infer their capabilities from vicarious and symbolic sources of efficacy information" (Bandura 1982, 128).

Finally, making an obvious but impor-

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tant point, Bandura writes that even where motivation can be assumed, perceived self-efficacy does not by itself determine whether coping behavior will be undertaken. Also crucial are judgements of the relevant environment. For example, one may believe oneself to be personally effective but feel that the environment inhibits or blocks the possibilities for successful action. With respect to either a prospective effort or a past performance, success or failure can be seen as dependent upon some combination of self-efficacy and environmental circumstance. And, of course, very often it can only be a matter of assumption as to what contribution is made by each part of that combination to an outcome (further discussion of this internal-external distinction can be found in Gurin and Brim 1984).

## Self-Efficacy in Political Life

Self-efficacy in political life has been the subject of field study for more than 30 years. Interest in this psychological disposition was inspired by the belief that it is a fundamental influence on both political participation and system support in a democracy. In the initial investigations, the concept was elaborated with differing emphases—"the sense of political efficacy" for Campbell and his colleagues (Campbell et al. 1960; Campbell, Gurin, and Miller 1954); "political self-confidence" for Janowitz and Marvick (1956); and "subjective political competence" for Almond and Verba (1963)—which led to considerable, and in some respects unfortunate, variation at the operational level. However, in the ensuing years, the Campbell conceptualization has come to be generally accepted in political science. Political self-efficacy, according to that formulation, is "the feeling that individual political action does have, or can have, an impact upon the political process . . . the feeling that political and social change is possible, and that the individual citizen can play a part in bringing about this change" (Campbell, Gurin, and Miller 1954, 187).

For the first two decades of research, the standard operationalization of political self-efficacy involved the following four survey items (statements with which respondents would either agree or disagree): (1) "People like me don't have any say about what the government does," (2) "Sometimes politics and government seem so complicated that a person like me can't really understand what is going on," (3) "I don't think public officials care much what people like me think," and (4) "Voting is the only way people like me can have any say about how the government runs things." However, in 1969, Mokken published analyses showing that item four did not scale with the others; efficacious respondents could disagree, arguing that voting was not the only way they could be effective, but at the same time inefficacious respondents could also disagree, arguing that not even by voting could they be effective.

Further evidence of need for modification in the standard operationalization came from papers by Hensler (1971), Converse (1972), and Balch (1974). These investigators demonstrated empirically what scholars had previously only argued (e.g., Almond and Verba 1963; Easton 1965; Gamson 1968; Lane 1959), namely that the measure of political efficacy commingled two separate dimensions, one focused on personal efficacy and the other focused on the responsiveness of government. This distinction matches Bandura's separation of perceived self-efficacy from perceived environmental circumstance. However, in the political science literature of that day, the latter dimension was too often muddied by conceptualizations emphasizing evaluation rather than perception. Thus, for example, Easton (1965) spoke of "support," Lane (1962) of "alienation," and Gamson (1968) of "trust."

It is true that perception of government responsiveness will often predict evaluation, perhaps especially in democratic systems, but nonetheless the two must be kept conceptually distinct (and, as will be seen below, viewing an environment as unresponsive does not, in Bandura's theory, necessarily lead to a sense of grievance). More recent work has done that (Abramson and Aldrich 1982; Finkel 1985; Pollock 1983). The terms *internal efficacy* and *external efficacy* have been embraced, presumably borrowed from Rotter's (1966) work on generalized expectancies for control. However, the latter term seems to me an unfortunate choice. Rotter wrote of the perception of external control—that is, control by outside forces or persons—over one's own reinforcements. External control has clear meaning, but external *efficacy* does not. Not even by connotation does the latter term connect to environmental circumstance, which is what is truly in point. Hence, I see Bandura's emphasis as much to be preferred.

It might be noted, too, that internal efficacy is different from internal control. The latter is a generalized expectancy, but for political scientists the former is a focused perception. Even in a particular domain, Bandura points out, the two may be uncorrelated. For example, one may believe that control over an event is in one's own hands, yet perceived self-efficacy can be low if one sees in oneself a lack of crucial skills.

The separation of judgments of political self-efficacy from those of political environment is important to the understanding of each part of this interacting combination. The two, in Bandura's theory, have quite different behavioral and emotional implications. When positive judgments of self are joined to positive judgments of the environment the result (motivation assumed) is "assured, opportune action," but when they are joined to negative judgments of the environment, the result is protest or milieu change, possibly accompanied by a sense of grievance. And when negative judgments of self are joined to positive judgments of the environment the result is self-devaluation and despondency, but when they are joined to negative judgments of the environment the result is simple apathy. That these distinctions can have important macroscopic implications is seen in the Abramson and Aldrich (1982) research tying the decline in U.S. electoral participation to a declining sense of government responsiveness—and, though they make little of it, a stable sense of political self-efficacy (for other systemic implications, see Gurin et al. 1969).

Extending Bandura's argument, one would expect that judgments of political self-efficacy and those of political environment would also have quite different resiliences. Positive political efficacy must often be psychologically joined to a more generalized sense of personal significance, a belief that one counts in this world (see Campbell 1962; Douvan and Walker 1956). Intuitively, one would expect such a sentiment to be both valued and defended by its holder, in which case, the evidence accepted by that holder as fully confirming could be of a weaker character—vicarious experience or verbal persuasion, in Bandura's terms, rather than actual performance attainments. And if an individual psychologically defends a sense of self-efficacy, that defense could be much strengthened by the fact that any performance failures (which, as noted, are often of ambiguous cause) may be blamed on environmental circumstance. Thus, in this theoretical view, belief in political self-efficacy probably would be more readily acquired and surely would be more actively defended than would any comparable belief about the political environment. In fact, the latter belief may in many instances be in service to the former.

The pertinent evidence on these points

is varied. Almond and Verba (1963) report that when a national sample of U.S. adults was asked if there was anything they, as individuals, could do about a national regulation that they disliked, 75% said that there was. Yet only 18% stated that they had ever undertaken such action. This ratio, about 4:1, is exceeded by the ratios for West Germany (5:1), Great Britain (7:1), Italy (9:1), and Mexico (12:1). It would seem that such statements of personal effectiveness must be grounded primarily in feelings of self-efficacy, but, in the absence of measurement, one cannot determine precisely what part is that and what part is perception of the political environment. It is clear, however, that whatever the compound, it is much less the result of performance attainments than it is of lessons learned in other ways.

Political socialization—in stable systems, normally a blend of verbal persuasion and vicarious experience—surely is important here. Easton and Dennis (1967) show in a sample of U.S. elementary school students that, by the eighth grade, more than 50% claim that their families have some influence in the political process. Jennings and Niemi (1974) show that among U.S. high school students, more attributed political influence to their parents than the parents did to themselves. Moreover, these students were more likely to claim understanding of politics than were their parents. The data indicate that perception of political self-efficacy is very high at the time of entry into the electorate, obviously not the result of performance attainments. But again one would wish for better measurement.

Good evidence supports the view that, once acquired, the sense of political self-efficacy and the sense of the political environment do have different resiliences. Gurin and Brim (1984), in the course of a general review of the personal efficacy literature, present several short time series of mean scores for the U.S. population showing substantially less variance for the measures of self-efficacy and greater variance for the measures of government responsiveness. And they give four-year panel test-retest correlations (i.e., stability coefficients) that in raw form run about .5 for the personal items but only about .2 for the environmental items. Based upon this and other evidence, they conclude that self-efficacy judgments are relatively impervious to events, an outcome they explain in functional terms. Psychological needs for control, for self-respect, and for consistency are posited, forces that on balance act to conserve the sense of self-efficacy, whether it be high or low. Disrupting this stability requires events that are psychologically "compelling"—that is, attention-getting and, in their implications for self-efficacy, incontrovertible.

Epistemological problems of functional theory aside, the Gurin-Brim argument leaves many questions unanswered. For example, can one know in advance what events will be psychologically compelling, and for whom? They apparently believe such events to be rare and idiosyncratic: "The paradox is that . . . events which make people look at themselves and deal with the self-implications of the experience are by definition too rare to alter a whole population's judgments of themselves as competent" (pp. 324-35). Thus, one should not expect to see any appreciable movement up or down in collective self-efficacy judgments, except perhaps over the very long run.

A second question has to do with the apparent belief that self-efficacy is conserved by psychological needs having equal force for individuals with either positive or negative self-feelings. This seems odd. It must mean that the need for consistency dominates the need for control. Might not an event that implied greater control be *for that very reason* compelling for those with negative feelings? And might not an event with the same force, were it to imply less control, be shrugged off by those with positive feelings? It seems reasonable to assume that the definition of a compelling event depends in significant part on the efficacy feelings of the beholder.

Gurin and Brim see perceptions of the system responsiveness as normally free from conserving forces—hence much affected by events—because of the absence of self-involvement. Change reflects a realistic appraisal of actual happenings. However, here too, a question of asymmetry arises. Katz et al. (1975) have asked what happens to the sense of government responsiveness after a direct test of one's political efficacy. In data from a nationwide survey study, they identified those who had in one way or another sought help from government. It turns out that those who had good experiences rated public bureaucracy no better than did those who had no contact whatsoever. However, those who had bad experiences rated the bureaucracy substantially below the average for the uninvolved. In other words, successful encounters with government do not seem to improve its standing, but unsuccessful encounters diminish it. Not given in the study, unfortunately, are any data on self-efficacy. It is clear that the asymmetry in these results does not easily lend itself to the belief that realistic appraisals are involved. More plausible is the view that even with judgments of the responsiveness of the environment, self-perceptions are intertwined in complex ways. Of course, that is only to restate Bandura's argument.

The present study brings new evidence to bear on the questions raised above. Using carefully tested measures of political self-efficacy and of government responsiveness, the research examines the consequences of successful and unsuccessful approaches to government officials in search of personal assistance. The results support the conclusion that performance attainments do have asymmetric effects both on perceptions of self-efficacy and on perceptions of environmental circumstance.

## Methods and Data

The data for this study come from a national sample survey of Indian male citizens, completed just after the 1967 national election. The particulars of sampling, questionnaire construction, field work, and data evaluation can be found in Eldersveld and Ahmed 1978.

The four standard political efficacy items were included in the questionnaire, accompanied in the same battery by three other agree-disagree measures. Sixty-five respondents who answered *don't know* to six or all seven of these items were dropped from the analysis. Another 577 respondents (29%) agreed with all seven. Tests with other agree-disagree items scattered through the questionnaire suggested that perhaps 120 of them were being acquiescent during the interview, with little or no attention to the specific content of each item offered in an agree-disagree format. However, since it seems that acquiescent respondents would not be among those with strong self-efficacy feelings, none of these respondents was ultimately deleted from the pool. Moreover, intercorrelations of the four items, with such cases removed, were only slightly smaller than when they were included.

Also in the questionnaire were two items that called for direct evaluation of government responses to personal needs. Both because these two items were located elsewhere in the questionnaire and because of their wording, no self-efficacy implications were likely to have been raised in the mind of the respondent. The first asked whether or not he was satisfied with the job being done by local-level officials, and the second asked the same with respect to higher-level officials.

Analysis of self-efficacy measures went through several steps. First, the four standard items were intercorrelated, revealing that items one ("don't have any say") and two ("can't understand") were the most closely related, with item three ("officials don't care") next and item four ("voting is the only way") the weakest of the lot. This ordering was the same whether the correlations were rank-order or product-moment.

Second, through the use of a variant of cluster analysis, "candidate" items—which now included, in addition to the four standard efficacy measures, the two items dealing with evaluation of government response and several other attitude variables—were tested against 50 demographic and attitude criterion variables to determine the extent to which the candidate items patterned together in their relationships with the criteria. Specifically, this involved (1) correlating each candidate variable with the full set of criterion variables, (2) entering these correlations into the clustering routine as the raw data for each candidate variable, (3) computing (and printing) the correlations of each candidate item's set of correlations with the counterpart set for every other candidate item—that is, distances between patterns were computed, and (4) combining the two items with the closest patterns by averaging, after which steps three and four were repeated again and again until all candidate variables were included in the final cluster. Obviously, it is the early clusters that are most useful here.

Put simply, this clustering approach (essentially that of Tryon; see Tryon and Bailey 1970) goes beyond the candidate items' relationships with one another and tests the extent to which their impacts on a variety of external variables are the same. The resultant findings were, first, that items one ("don't have any say") and two ("can't understand") patterned together very closely and, most remarkable, did so in every answer option, *agree, disagree,* and even *don't know.* Second, item four ("voting is the only way") was not related to the others. And third, after items one and two had been combined, item three ("officials don't care") did not cluster with them at all but instead patterned with the two variables measuring satisfaction with the job done by government officials.

Before construction of final measures, these data were also factor-analyzed. A self-efficacy factor could be extracted on which items one, two, and three loaded almost equally, but a government responsiveness factor also appeared on which item three loaded with the two measures of satisfaction with officials' performances.

Ultimately, the measure of political self-efficacy was derived from items one and two. These items met every test and correlated equally well in every one of the many demographic and language groups examined. The items plainly emphasize personal rather than systemic factors. Item three, on the other hand, carries external meaning in significant degree, and together with the two variables treating satisfaction with government, provides a good measure of government responsiveness. In final construction of the two measures, component items were summed and then standardized (means set to zero, and standard deviations to one). The two measures correlated at .27.

These final measures differ from those appearing in some of the literature on U.S. politics. There is no dispute about items two and three. However, Pollock (1983) and Abramson and Aldrich (1982) combine item four with item two in their measure of self-efficacy and combine item one with item three in their measure of government responsiveness. Gurin and Brim (1984) use item two as the measure of political efficacy and items one and three as part of a set of four variables defining responsiveness. Finkel (1985)

**Table 1. Perceived Self-Efficacy and Government Responsiveness by Status of Petition to Government**

| Perceptions | Petition to Government was Successful | Unsuccessful | Not Offered |
|---|---|---|---|
| Self-Efficacy | .19* | –.02 | –.02 |
| Government responsiveness | .09 | –.33* | .03 |
| Number of cases | 194 | 175 | 1,437 |

Note: Entries are mean scores on variables standardized for the entire sample. * marks each entry that is significantly different ($p < .01$) from its *Not Offered* counterpart.

finds in items one, two, and four the self-referent measure and in item three and two others (the same two "outside" variables as used by Gurin and Brim) the responsiveness measure. Craig and Maggiotto (1982) bring item one back to the responsiveness measure, as did Balch (1974). McPherson, Welch, and Clark (1977) dispute that assignment. Finally, perhaps needless to say, many different statistical techniques are used by these investigators in establishing their final measures.

Three things seem clear. The first is that continued inclusion of item four in either measure is not useful. Mokken (1969) demonstrates this in Dutch and U.S. data. Hensler (1971) and McPherson, Welch, and Clark (1977) demonstrate it in U.S. data. Now it can be seen in Indian data as well. The second is that additional standard items tapping each of the two perceptions—namely, of self and system—are sorely needed. And finally, because very different statistical approaches are likely to yield different multiple-item measures and, because of that, different and confusing estimates of relationships, exact comparison of findings from the efficacy literature (and perhaps from any political-attitude literature) are probably futile, without generally accepted procedures for scale construction.

A final point: in what follows, reference is made to a measure of political awareness. This measure was constructed exactly as were the two discussed above. Included are items dealing with political information and awareness of election events.

## Analysis and Findings

Asked if they had ever gone to an official in the government for help, those who said they had—and thus had put themselves in a position actually to test their perceptions of political self-efficacy—totaled 20% of the 1,805 cases in the analysis. They differ from the rest in their political awareness and education, both of which are significantly ($p < .05$), though not greatly, higher than average. This subset splits almost evenly into successful and unsuccessful groups, the two populations of interest. With respect to political awareness and education, the successful and the unsuccessful do not significantly differ from one another (the latter are a bit higher in awareness and a bit lower in education). However, with respect to judgments of self-efficacy and environmental circumstance, the differences are plain and can be seen in Table 1.

The successful petitioners typically show a sense of self-efficacy that is well above the norm for those making no such contact ($p < .01$) but a perception of governmental responsiveness that is only slightly and insignificantly above the

**Table 2. Perceived Self-Efficacy and Government Responsiveness for Successful and Unsuccessful Petitioners (Controlled for Education, Economic Circumstance, and Political Awareness)**

| | Petition to Government was | |
|---|---|---|
| Perceptions | Successful | Unsuccessful |
| Self-Efficacy | .13 (.01) | –.07 (.18) |
| Government responsiveness | .09 (.21) | –.31 (.01) |

Note: Entries are unstandardized regression coefficients. Following each entry, the associated significance figure is given in parentheses.

norm. Success is associated with very slight inclination to raise one's evaluation of government performance, a result matching that of Katz et al. (1975). However, that same success apparently enhances feelings of self-efficacy, an outcome in keeping with Bandura's evidence on the power of performance attainments.

Findings for the unsuccessful group are the reverse. On average, their efficacy level is indistinguishable from the noncontacters' norm, but their view of government responsiveness is very dramatically more negative ($p < .01$). In other words, self-efficacy suffers not at all, but the view of the government suffers a great deal (note, by the way, that beliefs about self-efficacy and about government responsiveness are here uncorrelated with partisan attachments).

These results do not change appreciably when success and failure are brought (as dummy variables) into a simple linear-regression equation in which education, objective economic circumstance, and political awareness are used to predict perception of self-efficacy on the one hand and of government responsiveness on the other. The unstandardized regression coefficients for the two measures of interest are given in Table 2. These coefficients, directly comparable to the simple means in Table 1, give essentially the same picture. Success significantly enhances the sense of self-efficacy but not the view of government. Failure changes self-efficacy very little but changes perception of government responsiveness dramatically.

Still another way to assess the effects of success or failure here is through consideration of a very simple social-learning model. In this model, education is taken as a predictor—and, in substantive terms, a producer—both of judgments of self-efficacy and judgments of government responsiveness. In other words, education is here taken to be a principal channel for political socialization, a view that has been argued with respect to Western democracies but has even greater persuasiveness in a context where-mass-media penetration is quite limited, as in the present case.

No socioeconomic variable has been more closely tied to political self-efficacy than has education (see Almond and Verba 1963; Campbell et al. 1960; Inkeles and Smith 1974). And none has nearly as strong an effect on self-efficacy in these Indian data. However, it is obvious that this variable indexes much more than education per se. For example, here it is correlated with wealth at .46, with caste at .34, and with father's education at .45—results that underscore its pronounced status coloration. Nonetheless, the education variable, with all its complexities, still stands as a reasonable proxy for the social-learning processes here of interest, namely verbal persuasion and vicarious experience of others' performance attainments. These processes drive what might be called normal political socialization, certainly with respect to political self-efficacy but probably with respect to many other political judgments as well.

The findings can be easily stated. First, the effect of education on self-efficacy for each of the three groups—that is, the noncontacters, the unsuccessful, and the successful—is close to invariant (the correlations, in order, being .31, .30, and .35). In spite of a direct test of political self-efficacy, this result suggests, the influence of social learning is only very slightly modified. Especially interesting here are the unsuccessful petitioners, seemingly as ready to be influenced by verbal persuasion and vicarious experience—that is, by socialization—as they would be had they never experienced failure in their contacts with government. The failure appears to have been dismissed *as a comment on self*, a result that again underscores the stamina of perceived self-efficacy.

On the other hand, the relationship of education with perception of government responsiveness is for any of the three groups quite trivial (the correlations being .05, .03, and .09, with the latter two not even approaching statistical significance). Socialization here matters very little. A government's good standing is not to be established so easily. Moreover, as shown earlier, even a successful encounter with government will not yield a great deal of credit. Once again, the possibility that perception of environment is *in service* to perception of self is suggested.

## Discussion

The evidence presented here supports four interrelated propositions: (1) that perceptions of political self-efficacy and perceptions of government responsiveness are arrived at differently, though not entirely separately; (2) that these judgments respond asymmetrically to events; (3) that a positive sense of self-efficacy is served by successful outcomes much more readily than is a positive sense of government responsiveness, and (4) that positive self-efficacy can be much more easily taught and maintained—in the sense that weaker evidence will suffice—than can positive perceptions of government responsiveness.

This evidence is striking not because it alone is conclusive but because it fits so well with other kinds of evidence on self-efficacy in several domains (see Bandura 1982, 1977; and Gurin and Brim 1984). Obviously, in the absence of such a fit, this cross-sectional survey study—like all such studies, lacking a really strong foundation for causal inference—would be of less import.

There is one recent study whose results may be read as at variance with proposition four above. Finkel (1985), using U.S. panel-study data, presents findings which suggest that acts of electoral participation have a small but significant influence on "external efficacy" and have no such influence on internal efficacy. Thus, it might seem that perception of government responsiveness is more easily influenced than perception of self-efficacy, at least by electoral participation. This research is carefully done but, regrettably, because of very different operationalization of the key concepts, lacks an exact (and perhaps even an approximate) basis for comparisons. The responsiveness measure is derived from items that include as key references Congress, elections, and parties. This puts a rather different spin on the measure, especially in the context of an election campaign, from that derived from references to "government officials." Less important, the self-efficacy measure in this U.S. study includes the item about voting being the only way to have a say in government. These problems serve as another reminder of the cumulativeness sacrificed in any research enterprise where measurement idiosyncracies are unresolved.

Consideration of the broader political implications of these findings brings us back to Bandura's theory, specifically to his findings on the interactive effects of self-efficacy and environment perceptions on motivation, emotions, and behavior. Recall that, in the absence of positive self-efficacy, individuals are found not to undertake action, whatever their views on the environment. Instead, they are apathetic (if they see a negative environment) or despondent (if they see a positive environment). In political life, then, one can conclude that both eventualities would mean the absence of participation (assuming voluntary participation rather than compulsory). Motivation to pursue self-interest in political or governmental arenas may not be wholly absent for such individuals, but it cannot produce action, conventional or otherwise. Although I can recall no example of the despondency case from the literature, the apathy case seems to me nicely represented by Banfield's (1958) citizens of Montegrano.

When feelings of efficacy and perceptions of environmental circumstance are both positive, action in response to suitable motivation is assured. In politics, if the relevant environmental actor is government, this combination of positive perceptions can be expected to induce broad participation in conventional political life (assuming, as before, personal motivation). And it probably would mean at least passive, and quite possibly active, support for the political system. Such sentiments and involvements have long been regarded as hallmarks of the good democratic citizen.

The last combination, positive self-efficacy with a negative perception of environmental circumstance, is seen to produce a sense of grievance, a "social activism," and protest actions against an unresponsive or negatively biased environment. For political life, again assuming the relevant environment is the political system, the implications would seem clear. One would not expect this to be a recipe for quiescent politics. However, the fact that in the U.S. this combination apparently is becoming more common with no observed consequence except declining voter turnout raises questions about this interpretation. It is possible that the answer lies in another consequence that Bandura sees as possible in this situation. "Should change be difficult to achieve," he writes, "given suitable alternatives people will desert environments that are unresponsive to their efforts and pursue their activities elsewhere" (1982, 141). No doubt such desertion is more likely to be psychological than physical. Thus, in the U.S. case, one might find that the decline in turnout represents such a psychological withdrawal from political life. Finally, it should also be noted that the grievance and protest that Bandura's theory predicts do appear in some careful studies of smaller scope (most notably, Gurin et al., 1969).

There are normative, as well as empirical, grounds for seeing perceptions of system responsiveness as important to the functioning of a democratic system. The surprising finding of the present research is that in contrast with a resilient sense of political self-efficacy, this crucial belief in system responsiveness seems quite fragile. Even when the system is in fact responsive, a benefitted citizen's inclination is self-congratulatory, crediting personal efficacy for favorable outcomes and discounting system performance. And if the system does fail to respond to the citizen's needs, its standing in his or her eyes falls substantially, perhaps so that a sense of self can remain intact. Realism about the capacities of both self and system is much to be desired. The obvious question is how such realism can be cultivated.

## Note

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# AGE AND TURNOUT IN NEW ELECTORATES AND PEASANT SOCIETIES

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*In established electorates in developed countries, voting turnout has a familiar curvilinear relationship with age. In new electorates, however, lack of experience and supposed resistance to new behavior among middle-aged voters suggest that turnout might be greater among the young. In peasant societies, the start-up costs that reduce turnout among young adults might not apply because of immobility of the population and resulting familiarity with local politics. Data from an early election in rural Kenya and from rural Turkey show that the familiar relationship holds even in these circumstances. The results suggest that increased electoral experience and resistance to newly available behavior do little to shape turnout. Mobility of the young, even in peasant populations, and psychological factors associated with aging are more powerful elements.*

The fact that "participation increases steadily with age until it reaches a peak in the middle years, and then gradually declines with old age" (Milbrath and Goel 1977, 114) is one of the most widely supported findings in the literature on political participation. Yet there remain some doubts on two fronts, first as to new electorates and second as to peasant societies.

For each of these special populations, we indicate why prior evidence about the age-turnout relationship is weak and give survey results that address the issue directly. There are three important findings: (1) the familiar turnout-by-age curve characterizes Kenya, the newest of new electorates; (2) an accounting of the Kenyan experience suggests that almost no electorates are totally new, which helps explain the first result; (3) the curve characterizes peasant populations in underdeveloped societies as accurately as it does the populations of the industrialized democracies. The evidence thus yields support for the near universality of this age-to-participation pattern and for the role of life-cycle factors rather than electoral experience in causing it.

## New Electorates: Kenya as a Case Study

In developed electorates there is little reason to challenge the familiar curvilinear relationship between age and turnout. Life-cycle factors (greater mobility of the young) and experience (the greater political experience of the middle-aged) coincide to produce an increase in turnout as one ages. In a new electorate, however,

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these forces conflict. Life-cycle factors still favor greater turnout among the middle-aged. But the experience hypothesis, at least in its extreme form, suggests higher turnout among the young. Middle-aged individuals in a new electorate have no previous voting experience to reinforce turnout behavior and, in addition, the absence of such experience may reinforce abstention with middle-aged citizens resisting the exercise of newly gained rights.[1]

The expectation of a possible decline in turnout is based on the assumption that new electorates are truly inexperienced in democratic politics. Therein lies the weakness of previous tests. In most previous tests (Niemi, Stanley, and Evans 1984; Black, Niemi, and Powell n.d.), the new electorate consisted of women whose husbands had long been eligible to vote and who themselves may have voted in earlier local elections. Spain is an exception, but there had been a brief period of Spanish democracy in the 1930s, and Spain was part of the developed Western world, in which voting and elections were commonplace. Thus a question remains: would the age-turnout relationship be downward-sloping if the entire electorate were new and lacking in previous voting experience?

African nations, moving from colonial status to independence in just a few years time, would seem to provide the critical test for the experience and life-cycle hypotheses. To a large degree, this is true. We will probably never find a better test of how "totally new" electorates respond to the voting experience. However, African history also shows that "countries [rarely] move in a single step from no past experience with democracy to free, contested, meaningful elections" (Niemi, Stanley, and Evans 1984, 384). A capsule summary of the development of electoral institutions in Kenya will substantiate this point and will help explain why, once again, we will find support for the traditional age-turnout relationship.

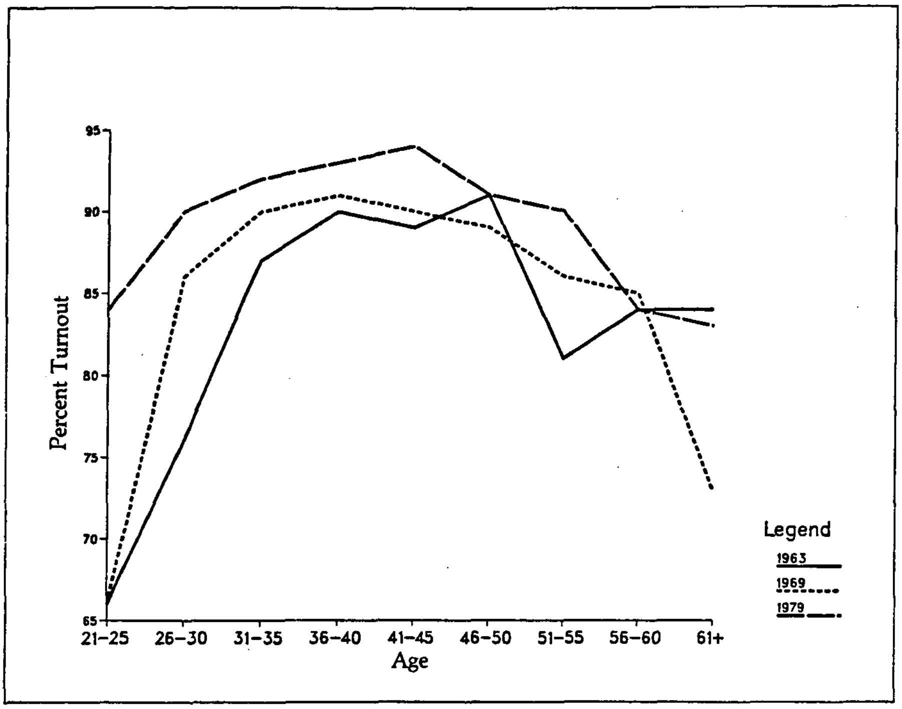
## The Historical Context

As was the situation in all of Britain's former African colonies and indeed all colonies in the British Empire, the electoral process in Kenya did not begin suddenly upon independence. Rather it was a process that was phased in, over a period of approximately 5 to 10 years, to transform an existing, appointed, quasi-parliamentary body—the Legislative Council (LEGCO)—into a fully and directly elected national legislature. Thus, any data on national parliamentary elections in ex-British Africa are not data on the first experience of an African public with elections, but "an early experience." The same is true, for somewhat different reasons, for national elections in ex-French Africa.

The evolution of parliamentary practice in Kenya was slow compared to that in Britain's other African colonies. Although the LEGCO first emerged in Britain's West African colonies as early as 1861, the appointment of the LEGCO by the colonial governor did not occur in Kenya until 1907. African participation in the LEGCO was further slowed by opposition from Kenya's powerful community of European "white settlers." Thus, the first African was not appointed to the LEGCO until 1944, and the first direct election of Africans to the council was delayed until 1957. White resistance to African representation resulted in the so-called "Mau Mau" Rebellion in the early 1950s, a guerilla insurgency by African nationalists that mobilized and politicized large portions of Kenya's rural African population and forced the British to hasten the transition to African rule. This politicization may explain why the turnout rates reported below for Kenya's second and subsequent national elections are unusually high and why there is little difference in turnout between the early and later elections.

The first parliamentary elections were held in 1957, with eight seats voted on by

Figure 1. Turnout by Age in Rural Kenya



the African population. Another six seats were contested in 1958. The first "true" general parliamentary election took place in February, 1961, 34 months before the country's independence. Half of the LEGCO was elected by the African population, and for the first time, Africans were permitted to form nation-wide political parties. Turnout was in the neighborhood of 12%.

The second general election was held in May 1963, after which Africans formed a government for the first time, with Jomo Kenyatta as prime minister. The British granted Kenya internal self-rule in June 1963, and total independence in December. Since independence, Kenya has held one "little" election in 1966 (a special election in certain constituencies) and four general elections. Turnout has ranged between 40% and 50%.[2]

## Turnout by Age

We present data for three elections spanning the first 16 years of Kenya's independence: (1) the second general election held in June 1963, six months before independence; (2) the third general election, in 1969; and (3) the fifth general election, in 1979. Turnout by age (respondent's age at the time of the election) for each of the three elections is plotted in Figure 1.[3] The familiar curve of rising and then falling turnout is strikingly evident in each one. The curve for the 1963 election contains no hint of a negative slope. In fact, it shows greater start-up effects among young people than either of the later elections, with even those in their late twenties voting at reduced rates.

Peak turnout did not occur until middle age, especially in 1963. The differences are small, of course, and pinpointing a precise maximum is more than the data can bear. Yet high and evidently increasing turnout into the forties underscores the degree to which an upward- rather than a downward-sloping curve characterizes new as well as old electorates.

Turnout declines among individuals in their fifties and beyond, but the drop-off appears in all three elections and not simply in 1963. The continued existence of this decline suggests that it is not chiefly due to resistance built up during years of disenfranchisement. Similarly, though turnout levels are inflated, they would not begin to approach the numbers shown if resistance to new experiences were a major factor among the elderly. Nor would turnout among the elderly exceed that among the very young, as it did by a wide margin in 1963.

The results are basically the same when sex and education are controlled, though, due to smaller numbers of cases, the curves become more jagged. Although the age of peak turnout varies, there is evidence of the curvilinear form nearly everywhere. Start-up delays are equally present among men and women and among the more and less educated. Drop-off is substantial among women and among those with no formal schooling. There is perhaps no drop-off among the formally educated, but this simply reinforces the point that there is no electoral resistance among the elderly as such; the usual decline can be overcome by education.

In short, the evidence on turnout in Kenya points to the presence of strong life-cycle factors and the absence or weakness of electoral resistance among the middle-aged and elderly. Effects of experience are not entirely absent; turnout did rise from one election to the next; and, as we noted, even the 1963 election was not the first electoral experience ever. But in the electorate as a whole, and even among women and the uneducated (members of the new electorate least likely to have been involved in preindependence political activities and therefore most likely to be hindered by inexperience), life-cycle effects predominate.

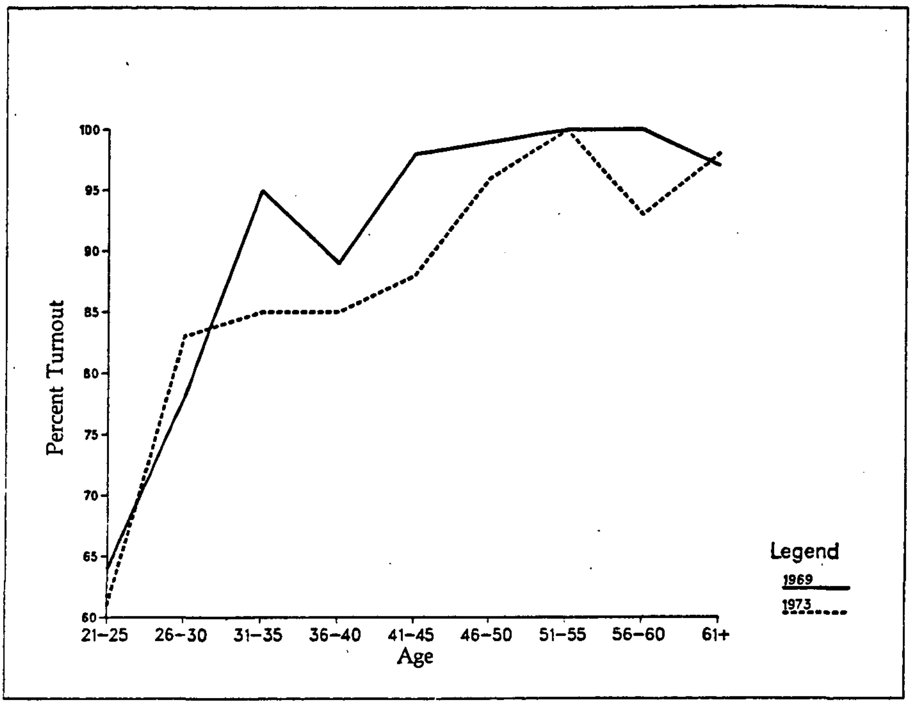
## Peasant Electorates: Kenya and Turkey

In many less-developed countries, roughly three-quarters of those residing in rural areas are small, peasant farmers. Such populations are thought to be sedentary and therefore familiar with local politics, characteristics that might make them immune to life-cycle effects ascribed to youthful mobility and inexperience.

As with the image of a sudden transition to democratic institutions, such a description is only half true. While rural populations are often well informed (Barkan 1976; Goel 1975, chap. 3; Hayward 1976), they are not so immobile. A substantial number of peasant farmers, particularly males between the ages of 20 and 35, seek seasonal or long-term employment outside their home areas. Population pressure on small land holdings, landlessness, declining per capita income from farming, and the desire to supplement farm income, all push young able-bodied males into the national labor force.

In Kenya the result is an almost perpetual process of periodic migration—mostly among young males—from the home rural area to other rural areas and to towns and cities, and back. Such migrants rarely become permanent residents of the areas to which they migrate, yet they are unlikely to vote in their home areas while they are away. In Turkey this same process has been supplemented since

Figure 2. Turnout by Age in Rural Turkey



1950 by heavy permanent migration to cities. Thus, peasant populations in developing societies are likely to show the same life-cycle effects as in developed countries.

Figure 1 supports this hypothesis with respect to Kenya. Figure 2 addresses the same point with data from parliamentary elections in rural Turkey.[4] The Turkish data reveal as striking an upswing among young adults as the Kenya data. Voters in their early twenties are especially scarce, and peak rates occur among adults in their forties and fifties. Initial turnout rates are particularly low for males, though turnout among women is also lowest in their early twenties and highest in middle age. Collectively, these data combined with data from Nigeria and India strongly indicate that the familiar turnout-by-age curve is equally characteristic of industrialized societies and of peasant populations across the Third World.

## Conclusion

In developed countries, a curvilinear relationship between age and turnout represents conventional wisdom; two new cases would not add to existing knowledge. But we have presented data of a different sort. The Kenyan data represent the best evidence we can muster on turnout in new electorates. As we saw, the recency of enfranchisement made no perceptible difference; the same familiar turnout curve emerged. The data from Kenya and Turkey are also fairly unique in that they represent behavior among peasants. Still further support for the traditional curve was the result.

Beyond the superficial similarity of shape, the reasons underlying the curve also seem to be similar across very different societies. Mobility of the young, for example, minimizes turnout even among heavily rural populations. Less obviously, increased electoral experience and resistance to newly available behaviors apparently do little to shape the turnout function. Psychological factors associated with maturing, which alter people's perspectives about politics and voting even in the absence of electoral experience, are apparently an important element. Overall, experience may play a role in increasing turnout as individuals move from early adulthood on into middle age, but it is the experience with life and with politics in general that does it rather than the specific experience of electoral politics.

## Notes

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1. Lane (1959, 125) suggested this by noting that women who formed their impressions of appropriate female behavior before passage of the nineteenth amendment may have continued to view politics as a "man's business." See also Campbell, et al. (1960, 488) and Wolfinger and Rosenstone (1980, 43). Converse (1969) incorporated a similar experience factor into his explanation of party identification differences across societies.

2. For more details, see Sanger and Nottingham 1964, Hyden and Leys 1972 and Barkan 1987.

3. The data are from surveys conducted by Barkan in 1974 and 1980. Turnout was recalled and therefore inflated, but we have no reason to suspect that misreporting is a curvilinear function of age. The surveys were conducted in totally rural districts populated primarily by small, peasant farmers (except for one urban district not used here). For details, see Barkan 1976.

4. The data were collected by Ilter Turan in 1974. Our analysis is limited to five administrative districts in which the population was classified as 85% to 100% rural. Reported turnout is higher than actual turnout. On the Turkish electoral system, see Landau, Ozbudun and Tachau 1980; Weiker 1981.

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# REVIEW ESSAYS

**South Africa without Apartheid: Dismantling Racial Domination.** By Heribert Adam and Kogila Moodley (Berkeley: University of California Press, 1986, 315 p. $18.95).

**The Militarization of South African Politics.** By Kenneth W. Grundy (Bloomington: University of Indiana Press, 1986, 133 p. $18.95).

**The American Connection: The Influence of U.S. Business on South Africa.** By Vic Razis (New York: St. Martin's, 1986, 246 p. $32.50).

It is not South Africa's racism but its legalization of racial stratification that sets the white state apart from all others. Furthermore, it was the employment of this legalized separation for the enhancement and expansion of political power that was critical in the development of modern Afrikaner hegemony. Imposing group membership from above, demarcating color-group boundaries, and confusing race/color with class, were all central elements in the transformation of a segregated but politically liberal white-run polity into a regime dominated by a narrowly based Afrikaner oligarchy determined to advance its own ethnic as well as national interests. Thus the first 20 years of Afrikaner control were marked by the subordination of individual to group or communal rights, the explosive growth of Afrikaner capitalism, and the creation of a vast bureaucratic and military-security establishment. Simultaneously, white South Africa was changing rapidly from an exporter of valuable but unfinished raw materials into a sophisticated regional industrial power capable of enhancing the value and the variety of its exports. The fuel for South Africa's economic emergence and reorientation in the 1960s and early 1970s came from Europe and the United States during a decade or more of high mineral prices and, internally, of seething indigenous discontent submerged beneath the repressive apparatus of a modernizing, increasingly secure, race-based autocracy.

The terms of world trade have turned decisively against South Africa. Except for gold and, possibly, diamonds, short- and medium-term trends in commodity prices are discouraging for raw materials producers. The black population of South Africa is growing at rates considerably higher than predicted rates of economic growth. With yearly population increases estimated at 3.6% or more, a rate exceeded in the world only by Kenya and Zimbabwe, South Africa is rapidly outstripping its ability to redistribute wealth or, critically, to provide opportunities and jobs for a black population that is impoverished and, in the later 1980s, will be capable of undermining the mechanisms long used by the apartheid regime to channel or deny indigenous aspirations.

Adam and Moodley perceptively suggest that the major aim of white South Africa in the 1980s has been to restructure (and symbolically reconfigure) itself so as to "forestall revolution." What they call "controlled change" has been employed to prevent (not to advance) fundamental change. They employ tactical readjustments (as others have called it) rather than strategic shifts. From the point of view of most white South Africans (fewer than 5 million in a nation now totalling about 30 million but expected to approach 50 million in 2010 without much growth in white numbers) and few blacks, apartheid is being dismantled by fiat and inadvertence and in the face of massive skilled-labor shortages caused by a systematic neglect of black education and training.

Given the wave of unprecedented protest that has convulsed South Africa's townships since September 1984, which three successive states of emergency have failed to control, it is evident that what Adam and Moodley like to call "technocratic liberation" has not and cannot provide a cure acceptable to Africans. Blacks across the urban-rural spectrum in South Africa have become sufficiently politicized and mobilized to demand—and to refuse to accept anything less than—solid increments of real political power. A full franchise in a unitary state is their demand. The white state, already prepared to concede social and economic enhancements, is as yet unready (despite pressure from big business to do so) to connive in its own loss of power. Relying, as the state does, on the best trained and most cohesive military establishment in Africa, it expects to control and dictate the pace of reform, all the while eroding white privilege but not white power. The mailed fist alternates with, and is often masked by, a velvet glove.

The regime will not soon collapse. A general rebellion will not soon sweep whites out of power. The security apparatus is white and determined to prevent any nonnegotiated transfer of power to blacks. Grundy's slim but effective study of the role of the military in South African politics is an expanded version of a monograph published in South Africa. It emphasizes the loyalty of the security apparatus to the white state, explains its newly important role and position in the oligarchic-bureaucratic apparatus that rules South Africa, and stresses the decisive interventionist role of the military in the southern African region. Destabilization is a military tool with profound foreign policy implications. South Africa has extended the effective boundaries of its hegemony beyond the Zambezi and Kunene rivers into Angola and Zambia. Mozambique, once a Soviet-aligned state, has become a substantial client of South Africa. As important as Grundy's exposition is for the policy debate over South Africa, his is not an examination of South Africa's military capabilities.

Adam and Moodley provide a good Baedeker to the contradictions and contortions of today's South Africa. They explain the state's ability to coerce but misunderstand the role of legitimacy, manufactured or not. They are helpfully taxonomic, separating Afrikaner political tendencies into four sensible categories rather than the usual two and, less persuasively, dividing the African political universe into another four segments (the Zulu Inkatha movement is labelled "pragmatic institutionalist" and other homeland organizations lumped with the "patronage-client" urban-community councillors). Their short section on political violence in the townships offers an analytical dimension that transcends the usual journalistic explanations. However, their equally brief discussion of the liberation strategy of the banned African National Congress (ANC) breaks no new ground. The emergence of African trade unions is a recent phenomenon and one that has obvious political implications in a deeply cleaved society like South Africa. Adam and Moodley, acute observers of South Africa, provide a preliminary guide to the bourgeois, crypto-Marxist, and reformist elements into which the labor movement can be divided.

Corporate enterprise is already searching for a compromise between white-official and black-nationalist positions. English-speaking magnates now enhance the creditability of the ANC. Even Afrikaans-speaking business leaders understand how important it is to coax an unwilling oligarchical leadership to the bargaining table. In this effort, U.S. multinational corporations have played far less important roles than their South African counterparts. But the U.S. firms have been in the vanguard of expanding black opportunities and, lately, of exploring methods of political intervention. The U.S. connection is comparatively small—5% of total assets and 1% of total wage employment—and no longer dominates industrial sectors. Razis offers a survey of the position of U.S. business in South Africa, the impact of the code of conduct introduced by the Rev. Leon Sullivan, and helpful statistics on trade, dividends, capital flows, and so on. Alas, his analysis and arguments have largely been overtaken by events—by the economic recession in South Africa, by accelerated U.S. corporate withdrawals, and by U.S. sanctions legislation.

Adam and Moodley are fundamentally optimistic about the prospects for a relatively democratic society in a fully integrated future South Africa. South Africans, they say, can "enjoy the future." They remind readers that unlike so many other deeply divided, ethnically plural societies, South Africans are separated by legally mandated social distance rather than by cultural distance. Religion unifies more than it divides South Africans. Economically and geographically, South Africa is and has always been essentially integrated and thus interdependent. The authors reject the application of Arend Lijphart's consociational formula to South Africa and criticize those who see virtue or possibility in partition. Given Afrikaner opposition to non-racial winner-take-all voting and denying Samuel Huntington's assertion that racial communities can have "moral claims and rights," they offer a power-sharing model as a best solution, either one based on Carl Friedrich's corporate (nongeographic) federalism and freely chosen group membership or one with regional components—or some mixture of the two. Admittedly, the ANC would reject such an answer (and so would many politicized Africans). Constitutional engineering could, Adam and Moodley hope, bridge this gap, possibly by vesting power in more than one parliamentary house and combining direct representation in the lower house with ethnic or group representation in the upper one. Unfortunately, the authors do not provide a blueprint, nor, in their largely impressionistic essay, are they more than vague about how such a solution could be attained. To reject the technocratic notion that the white state can continue imposing its will indefinitely on insurrectionist blacks is wise; to suggest, as these otherwise perceptive thinkers do, that whites will negotiate and blacks will accept a structured compromise that shares rather than transfers power is utopian. Africans rightly or wrongly believe themselves empowered and their cause legitimate in a way that is strikingly new in South Africa.

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**Trouble in Paradise: The Suburban Transformation in America.** By Mark Baldassare (New York: Columbia University Press, 1986. 251 p. $25.00).

**The Progressive City: Planning and Participation, 1969-1984.** By Pierre Clavel (New Brunswick, NJ: Rutgers University Press, 1986. 262 p. $28.00, cloth; $10.00, paper).

**Organizing the Movement: The Roots and Growth of ACORN.** By Gary Delgado (Philadelphia: Temple University Press, 1986. 269 p. $24.95).

**Middle Class Radicalism in Santa Monica.** By Mark E. Kann (Philadelphia: Temple University Press, 1986. 322 p. $24.95).

The urban-politics literature has increasingly focused on conflicts between progrowth forces and their opponents. Through the work of Harvey Molotch, John Mollenkopf, William Domhoff, Clarence Stone, Paul Peterson, and others, the power of the "growth machine" has been described and explained. Much less has been written about the forces on the political Left that have challenged the power and policies of the growth machine. Clavel, Kann, Delgado, and, to a lesser extent, Baldassare examine not only important sources of discontent with growth machines but the "progressive" and "radical" alternatives that have materialized in U.S. communities during the seventies and eighties. Clavel, Kann, and Delgado are clearly sympathetic with the ideals of the Left and demonstrate that the mobilized Left has successfully implemented a variety of progressive policies, but they have retained sufficient objectivity to describe the shortcomings and dilemmas of the progressive-radical Left.

Baldassare, a sociologist at University of California, Irvine, draws upon citizen surveys conducted in Orange County, California and completed in 1982 and 1983 to discuss problems confronting and changing suburbia. Baldassare's focus is thus on suburban transformation. But because issues concerning urban transformation and economic growth are strongly intertwined, he makes several contributions about antigrowth forces. He finds, for example, "a broad awakening of sentiment against growth, and increasing skepticism that growth offers the average person a better life." Baldassare's data show that citizen opposition to growth is rooted primarily in environmental concerns and secondarily in economic ones and is enhanced by a growing distrust of local governments. Baldassare argues that even small protest groups can successfully challenge weak, fragmented suburban governments and that developers often take their projects elsewhere when citizen opposition arises. Baldassare finds, however, that opponents of growth have mixed views concerning the appropriate policies to achieve growth limits. As a result, there is no well-defined group of citizens constituting a no-growth movement, and opposition to policies promoting growth must be mobilized on an ad hoc, issue-specific basis.

Delgado, a founding member of the Association of Community Organizations for Reform Now (ACORN) and a sociologist, argues that ACORN emerged from the movements of the sixties (especially from the welfare rights movement) to assert "the right to stay put." When corporate interests, aided by federal and local governments, sought to reappropriate urban space from the poor, ACORN resisted urban renewal, fought evictions, and protested service shrinkages intended to force the poor to retreat from urban centers. Delgado argues that discontent with the growth machine, especially among the poor, must be organized to have an impact on the political process and that such organization requires substantial direction by white, middle-class professionals. Although Delgado is clearly familiar with a variety of theoretical perspectives, his concerns are those of the pragmatic organizer. Thus, through his description of the ACORN model, one learns a good deal about how to organize and win in battles with the growth machine. Nevertheless, political scientists may find more interesting his analysis of issues confronting community organizations (COs)—whether COs should move beyond concrete, material issues to broader ideological stances involving social justice, whether COs should seek alliances with other movements on the Left, and whether COs should move from militant protest on policy issues to electoral activity.

Pierre Clavel, a city and regional planner at Cornell, argues that growth coalitions became unstable during the 1960s. Drawing heavily on Theodore Lowi's arguments in *The End of Liberalism* (New York: Norton, 1979), Clavel argues that urban governments seldom pursue their "substantive" growth policies but simply make compromises on policy issues and delegate authority. However, he claims that cities have an alternative to growth-oriented and nonsubstantive government. They can pursue the progressive programs and policies that emerged in Hartford, Cleveland, Berkeley, Santa Monica, and Burlington. His book describes how elected officials and city planners in these cities have limited property, rights, experimented with municipal ownership, supported housing cooperatives, collected revenues and delivered services on a more progressive basis, militantly advocated for citizens against suburban and business interests, and encouraged citizen participation. Clavel's basic argument is that these progressive cities succeeded in forcing progrowth interests into providing significant benefits to local residents as the price of doing business in their cities. Choosing not to theorize extensively about his cases, Clavel adopts a "reporting" approach that focuses mainly on progressive leaders. While this yields interesting insights into the opportunities and problems confronting progressive politicians, the political base of support for such progressive leaders is inadequately explored. While Clavel claims in his conclusion that the successes of progressives were due to the interplay of "the people" and "professionals," the impression that emerges from Clavel's case studies is that progressive change is due to the happenstance election of progressives rather than to broader citizen movements.

A more theoretical analysis of the challenge to the growth machine by the Left is that provided by Mark Kann, a political scientist at the University of Southern California, who focuses on the ascent to power by the Santa Monicans for Renters' Rights (SMRR) coalition. While Kann provides an interesting detailed history of the limited-growth movement in Santa Monica, his contributions are enhanced because of his ability to integrate into his case study many ideas from the writings of U.S. democratic theorists and intellectuals on the Left. His major contribution is to show that the affluent middle class, which dominates Santa Monican politics, provides a fertile base for mobilizations against the growth machine and for radical goals generally. The traditionalism of small businessmen, the idealism of new professionals, and the self-interest of renters and homeowners threatened by the costs of rapid growth make these elements within the middle class receptive to the idea of "human scale community"—small social units, rooted in local traditions and controlled by local citizens. By mobilizing these interests over the issue of rent control—an issue made possible by skyrocketing real-estate values following completion of the Santa Monica expressway—the SMRR was able to gain control of city hall in 1981.

Kann shows that radicals in office had to manage the tensions between their principles and practical politics. These tensions were especially strong regarding the ideals of participatory democracy (as citizen involvement and neighborhood empowerment were too often sacrificed to the need for professionalism and "good government") and economic democracy (as the environmental and consumption concerns of the middle class received higher priority than did concerns for social justice). Although these failures have prompted other observers to view the Santa Monican experiment as a typical case of liberal reformism, Kann makes a compelling case that Santa Monica represents efforts to institutionalize ideals of the New Left and that the tensions experienced by Santa Monican radicals illustrate a broader U.S.-middle-class ambivalence regarding the conflicting demands of material success and political ideals. His analysis thus ends in the argument that the middle class provides a base for ending the dominance of the growth machine and for building, through the slow resolution of idealistic and pragmatic tensions, a social movement toward a one-class society that permits, but regulates, markets and private property so that everyone has economic self-sufficiency and enjoys some political power.

In summary, Baldassare, Delgado, Clavel, and especially Kann make important contributions to our understanding of the coalition of interests that can be mobilized against the growth machine. They tell us the social groupings that oppose growth politics, their concerns, their ideals and policy goals, their oppositional strategies, and the problems they encounter when given the opportunity to govern. The major limitation of these studies is their tendency to focus solely on the Left, rather than on the interaction and conflict between progrowth and antigrowth forces. In other words, just as the earlier literature tended to focus on the actors within the growth machine and underplay the political role of its opponents, so these studies tend to focus on the antigrowth actors, underplaying the continuing power of the growth machine. This leads the authors to explain the policy making and electoral failures of radicals in terms of the radicals themselves—their failure to articulate an ideology of social justice, their failure to empower citizens, their failure to move beyond self-interest, and so forth. Without knowing more about the counteractivities of the growth machine and how the broader public reacts to the conflict between the growth machine and the Left, the sufficiency of such endogenous explanations is unclear.

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**Europe and the Superpowers: Political, Economic, and Military Policies in the 1980s.** Edited by Steven Bethlen and Ivan Volgyes (Boulder, CO: Westview, 1985. 230 p. $20.00).

**Shattering Europe's Defense Consensus: The Antinuclear Protest Movement and the Future of NATO.** Edited by James E. Dougherty and Robert L. Pfaltzgraff, Jr. (Washington: Pergamon-Brassey's International Defense Publishers, 1985. 224 p. $19.95).

**Semialignment and Western Security.** Edited by Nils Ørvik (New York: St. Martin's, 1986. 286 p. $35.00).

All three books under review purport to contribute to our understanding of Western security and transatlantic relations. Two of them focus on particular aspects, such as the alliance behavior of small nations and the antinuclear protest movements in Europe, but extend their perspective far beyond that. All three concentrate primarily on Europe, while only one of them actually looks at the *whole* of Europe—both East and West—and considers Western security in reference to the political developments in and behavior of all the main entities concerned; the United States, Western Europe, Eastern Europe, and the USSR. For the other two books, Eastern Europe and the USSR are reduced to the Soviet threat, and Western security is more or less reduced to the need for Western European allies to align with U.S. policies. While too wide a scope has its analytical limitations, the kind of more narrow scope outlined above certainly has more of them. This is particularly true when sufficient care is not made to discuss the political salience of the particular aspects on which more far-reaching conclusions are based.

*Semialignment and Western Security* contains analyses of the alliance behavior of Denmark, Greece, the Netherlands, Canada, and Norway. As a general introduction, Ørvik has written a chapter inspired by the realist school, maintaining national security as a major concern and the pursuit of national interests as the fundamental basis upon which states operate. While emphasis on national sovereignty and autonomy is an overarching goal, alignment or nonalignment are the means. However, Ørvik asks whether there is a *third* option "in between—a "grey area" in which a state may formally take part in an alliance but not play by the rules implied by alliance membership. Can NATO be an effective deterrent if some nations opt out? Where is the breaking point as to NATO's credibility?

Ørvik defines "semialignment" as the practice of nonaligned policies within an organizational framework of formal alignment. His personal condemnation of this practice is obvious, as it is seen as weakening NATO's deterrent and, consequently, Western security. Three basic criteria are set up to operationalize the requirements on which NATO's success depends: (1) that national contributions be in reasonable proportion to a member's capabilities and resources; (2) that no member obstruct the alliance from fulfilling its objectives; and (3) that no member engage in actions or omissions that may endanger the security of other members (p. 9).

Regrettably, this interesting point of departure is seriously undermined by numerous analytic flaws and Ørvik's personal beliefs about what NATO *ought* to be. To Ørvik, NATO's purpose is *military deterrence*, nothing else, despite the Harmel Report's endorsement (still in effect) of the "dual tracks" of deterrence *and detente* as NATO's official policy goals. As for the analytic flaws, these are too numerous to enumerate. Among the most serious is Ørvik's implicit notion that only *small* nations can be "semialigned" (France is dismissed as a potential example at the outset, because of her great power status and independent nuclear forces). Ørvik frequently mentions that many small nations sometimes stick to their own views and evaluations and refuse to submit to the "NATO leadership." The latter term is never defined but seemingly means the United States. Yet Ørvik does not consider the possibility that U.S. "global unilateralism" vis-a-vis its European allies may turn out to be another word for Ørvik's "semialignment."

Another serious flaw is that Ørvik seems to ignore the notion of "small state policies" (cf., Maurice A. East), according to which there is a certain pattern of foreign policy behavior that is *rational* for small nations to pursue and that is clearly linked to their national interests and capabilities. What Ørvik laments as policies that disintegrate the Western alliance and play into the hands of the USSR—and which he considers irrational ("semialignment cannot be fully explained in rational terms," p. 249)—may be well suited to a nation's understanding of its interests and balance various policy trade-offs quite well. Of course, the effects and potential implications of the foreign and security policies of all nations—large and small—should be subjected to systematic analysis. But a good starting point would be to examine how each nation's leaders have perceived and evaluated their own national interests, trade-offs, and options. Ørvik, however, substitutes his own personal and normative views. The result is not an example of good political science!

Despite a highly biased general framework, the authors of the chapters on Denmark, Greece, the Netherlands, and Canada do a good job in describing, analyzing, and evaluating the alliance policies of their respective nations. It is illustrative that they have joined in writing a *separate* concluding chapter that stands out in sharp contrast to Ørvik's personal concluding chapter. They stress NATO as a consensual association of states that see each other as equals, and accept as a matter of fact that small powers are able to calculate their own interests as well as major powers. Ørvik, on the other hand, states that "the smaller nations should feel more at home in NATO than in any other existing international organization" (p. 249), and laments that "the semialigned still seem almost frivolously unaware of the relationship between military power and their national security" (p. 263). In short, in this reviewer's opinion, the value of the book lies solely in the balanced, informative, and perceptive evaluation of the problems of alliances—emphasizing the different geopolitical situations for small versus large states—which the authors *not* named on the front cover have contributed.

To conclude this critical review, Ørvik's chapter on Norway deserves a few separate comments. Ørvik says the "national interests" represent the fundamental guidelines for every state, yet he sets himself and his own views *above* the policies and judgments based on a broad consensus in Norway for almost 40 years. His chapter is predominantly a polemical piece written under academic disguise.

*Shattering Europe's Defense Consensus* describes and analyzes the recent West European peace movements, with a particular focus on their domestic roots in terms of cultural, social, and political variables as determinants. Following two introductory chapters, the antinuclear movements in Great Britain, West Germany, the Netherlands, Italy, and France are treated—a couple of them somewhat too descriptively, but the majority with a thorough analysis of the various roots and idea elements in the country concerned. This analytic perspective, in fact, is the most valuable aspect of the whole book.

A few critical comments are in order. The authors seem generally unsympathetic to the movements under analysis. This position is fair enough but should, perhaps, be based on an evaluation of the potential *consequences* of the proposed or implicit policies rather than on the likes and dislikes of specific opinions and political groups. Furthermore, while in his introductory chapter Dougherty rightly points to the nuclear weapons issue (INF) as the main core for the various peace movements' mobilization and coalescence, neither he nor the other authors pay much attention to how U.S. policies, particularly under the Reagan Administration, played into the hands of the antinuclear movements and other "peace groups." The demise of detente as a policy objective in the United States policy toward the USSR, the nonratification of SALT II, belligerent rhetoric in Washington; such topics are virtually nonexistent in the explanation of *why* these movements became such a political wave in many European countries in the early 1980s. Carter's unfortunate hand with the so-called neutron bomb issue is pointed out, but the uninformed reader might easily get the impression that after Carter there was a calm, steadfast policy line in Washington, constructively facing up to defeatist Europeans as well as foiling Soviet plots to lure Western Europe away from the U.S. The book, therefore, offers neither a bipartisan perspective in terms of U.S. politics nor a valid perspective on the political dynamics in transatlantic relations. It is indicative that neither *SALT* nor *Detente* are found in the book's index. The SALT-II treaty is not even mentioned in the text, despite the political significance in Europe of its abandonment.

The lack of an overall and valid perspective on transatlantic relations is the main problem of this book. The analyses tend to regress into an ethnocentric U.S. pattern that considers Europe as an "object" oscillating between the "West" (i.e., the United States) and "Angst," "pacifism," and "neutralism." Only James B. Foley's chapter on France conveys an impression of the Europeans as political *actors* in their own right, pursuing their own interests with their own political strategies. Pfaltzgraff's concluding chapter treats the overall transatlantic issues as a process among different actors with differing views; but his discussion of the potential long-term impacts of the peace movements in Europe remains too vague, despite a number of other important observations.

The book promises to contribute to a better understanding of the future of NATO. Jacquelyn K. Davis's chapter on the future alliance strategy and force structure does not deliver. While paying considerable attention to the breakdown of consensus in Europe in the aftermath of INF, she disregards the effects of the breakdown of consensus in the United States after Vietnam. She is worried about the activists in Europe who "chip away at the defense consensus that for so long served Western interests so well" (p. 202), while ignoring the fact that since 1967 official NATO policy has been deterrence *and* detente, which makes it part of the current basis upon which NATO's future strategy will have to be built. It is highly unlikely that this "dual track" strategy can be altered without causing a rupture in U.S.-European relations.

In summary, *Shattering Europe's Defense Consensus* is valuable as a contribution to a better understanding of the *domestic* roots of the antinuclear movements in Europe, but is not a very informative book as to transatlantic relations and the future of NATO. The breakdown of detente in the late 1970s produced unilateralism on the Right in the United States and on the Left in Western Europe. The unilateralists gained considerable political power in Washington and much less in Europe, where they are still out of power. Nevertheless, Western Europe is not simply an "object" in a contest between the United States and the Soviet Union.

*Europe and the Superpowers* includes the kind of overall and comprehensive perspective that was noted as missing in the previous book. As a collaborative effort between U.S. and European scholars, it also presents a more balanced and varied discussion.

In the introduction, Volgyes stresses the need to be "open-minded and sensitive to the perspectives of the policy-makers guiding the various nations of the alliance system" (p. xi). This reviewer certainly agrees.

Books consisting of papers that have been presented at a conference frequently have too many points of departure to form a whole. This is the case here, although less so than might have been expected. There is a certain discrepancy among the contributions. Some chapters constitute regular length papers while a few at the end of the book seem more like summarized discussion points. Obviously, the topic of the book, with its many dimensions (cultural, political, economic, and military) and four basic "entities" (the United States, Western Europe, Eastern Europe, and the USSR), is too vast for a full treatment. But what is covered is of a high quality and constitutes worthwhile reading.

This reviewer found George Schopflin's and Wolfram F. Hanrieder's contributions particularly useful. Schopflin applies a broad cultural and historical perspective and achieves a delineation of the "European Identity" in politically fundamental terms. Hanrieder's chapter, "Arms Control and Security: The Federal Republic and the European Order" is exemplary. Especially for those who simply state that the United States deployed new intermediary nuclear weapons in Europe "because the Europeans asked the United States to do it," Hanrieder's analysis of what happened *before* the December 1979 NATO decision should be required reading. As he points out, it was Carter's unwillingness to incorporate INF within the SALT-II framework, combined with the enhanced significance of INF imbalances in Europe in the light of codified strategic parity between the United States and the USSR, that triggered the need to make INF a political issue. And the Soviets' deployment of SS-20 certainly made it rather precarious. Hanrieder also points to the European goals of a *new European order* as crucial for understanding the Western European side of the Atlantic relationship.

This book is highly recommended. William Safran's chapter, "Western Europe and the United States: Reciprocal Diffusion, Convergence, and the Prospects of a Non-Military Atlanticism" adds to the many articles and chapters written on U.S.-European relations lately. He points to the importance of culture, though he neglects to note that the recent "Americanization of America" is paralleled by the "Europeanization of Europe." Several chapters on Soviet policy and Eastern Europe are well written and thought-provoking. Angela E. Stent's analysis of East-West economic relations as a West-West problem deserves credit, too. She rightly points to the difference between U.S. and European definitions of security and to the contrasting U.S. and European views on the linkage between politics and economics. In this regard, Hanrieder's point about the European desire for a new European order is also pertinent.

In summary, *Europe and the Superpowers* presents a picture of the considerable potential for dynamic change in Europe, while the constraints represented by some U.S. and most Soviet policies are entered in the equation as well. And although the equation has no obvious solution, one conclusion clearly can be drawn: there is a need for continued transatlantic cooperation in the field of military security for the foreseeable future.

BAARD B. KNUDSEN

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**Surveying Subjective Phenomena.** Edited by Charles F. Turner and Elizabeth Martin (New York: Russell Sage, 1985. Vol. 1, xvi, 495 p. $37.50; Vol. 2, xvi, 616 p. $47.50; Two-volume set, $75.00).

This valuable report of a panel of the National Research Council (NRC) organized by its Committee on National Statistics can be read at three levels. Each is important. It is, first, an NRC report, complete with analysis of a question—how subjective measures are and can better be used in survey research—and recommendations. More than a report, these two volumes offer state-of-the-art commentary on many central issues in survey research and methodology. In this connection, do not be misled by the title, which suggests a narrower focus than what is, in fact, covered. Finally, and as nearly as this reviewer can tell, unintentionally, the work is a powerful statement about the conditions under which empirical social science has established itself over the last half century.

Before commenting on each of these dimensions in turn, a covering advertisement is in order. If the political scientists and sociologists likely to read this review believe that better survey measurement of subjective phenomena is an arcane topic, of interest to survey methodologists but of no larger relevance, they are wrong. We improve our analysis of fundamental issues—political legitimacy, social tension, ethnic identity, rational choice—as we improve measurement, including, though not limited to, survey measurement of the sort defended, elaborated, analyzed, and pushed forward in the volumes under review. The stakes matter, and methodological advances will make a difference. How useful, then, is the work of this particular NRC panel?

The reader primarily interested in this work as an NRC report will concentrate on the 30 pages or so of recommendations. There the reader will find sensible arguments that, if tended to, would improve the climate for the conduct of polls and surveys, would upgrade current survey methods and practice, and would advance the scientific use of survey data. For readers familiar with the struggles to advance on these fronts—dating to the Social Science Research Council (SSRC) committee formed when the pollsters failed to anticipate the outcome of the 1948 presidential election—the recommendations present few surprises. But the authors present them in a forceful and unusually instructive manner, adding in the process some fresh arguments. Persons less familiar with the ongoing efforts to professionalize the collecting and reporting of survey data will not find a better review of the key problems or a clearer statement of what can be done to address them.

Skeptics who belittle panel reports and their endless recommendations, doubtful of their capacity to influence anything, should read more closely the record of the past four decades. Serious progress has been made on many fronts: the statistical analysis of categorical data, the treatment of nonsampling error, even public education. Note, for instance, the responsibility with which reputable pollsters now report their findings, drawing the readers attention to sampling error and (unknown) biases resulting from measurement and survey procedures. The skeptic, if intellectually honest, will recognize in the recommendations of this NRC report the substantive and political basis for forward movement in improving survey-data collection and its use by society.

Incidentally, though of particular interest to political scientists, the only (identified) issue on which the panel failed to reach agreement was not methodological but political in content. The panel disagreed on whether it is proper for federal agencies to survey citizens on their feelings, beliefs, and attitudes. Unfortunately, what we may presume was a lively debate weighing the claims of citizen privacy against social knowledge is but briefly and inadequately reported.

If 30 pages focus on recommendations, more than 900 pages, including summaries of original research prepared for the panel, offer state-of-the-art analysis of many (not all) key issues in survey measurement and practice. No scholar interested in the methodology of surveys can afford to ignore these materials, especially the sections that advance original research. In addition to treating the uses and abuses of surveys, volume 1 is the panel's statement on measurement issues and the survey-interview process. Volume 2 contains 18 individually authored papers, ranging from fascinating historical material to original analysis, such as "why surveys disagree." Indeed on a topic that has long vexed the survey community (and led to public skepticism toward surveys), namely, "house effects," or why the same survey instrument administered by two reputable organizations produces different estimates of population characteristics, the materials in this study now constitute the definitive statement. And for the instructor who has unsuccessfully wrestled with how to explain to students the differences between subjective and objective measures, the introduction to volume 1 coupled with material drawing on recent work in cognitive science (reported in chapter 9) will be much appreciated. Also of value is the treatment of the survey enterprise itself—its growth, its academic, commercial, and government roots, its personnel and organizations, and more.

Of course, not every topic is covered equally well. This is, after all, a committee report, written under time pressure and constrained by budgetary limitations. Consequently, it is subject to the unevenness that afflicts such products, particularly noticeable in the topics covered in volume 2. But the editors, Charles F. Turner and Elizabeth Martin, have fashioned a coherent and readable product, one far superior to what we normally expect of a committee report. In the judgment of this reviewer, the work holds up admirably even if measured against more exacting standards. More than a committee report, it is a first-rate textbook and an intelligent compilation of materials relevant to assessing how far we have come, and how far we have yet to go, in measuring and interpreting subjective phenomena.

I remarked earlier that these two volumes could be read at three levels, though the third level either went unnoticed or was deliberately neglected by the editors and authors. The story unfolds of remarkable advances in the careful measurement of population and subpopulation characteristics through survey data over the last 50 years, and with it, advances in empirical work in the social sciences. What struck this reviewer so forcefully were the heroes of this story. There were, to be sure, individuals who made key contributions—Thurstone, Stouffer, Lazarsfeld, and, in more recent years, Bradburn, Cantril, Duncan (who chaired the committee producing this report), Goodman, Kruskal, and Schuman. But as I read the story, the real heroes have been institutions: American Association for Public Opinion Research (AAPOR), the Census Bureau, the Gallup organization, the Institute for Social Research, the National Research Council, National Opinion Research Center (NORC), the Social Science Research Council, and the various data archives such as the Inter-University Consortium for Political and Social Research (ICPSR) and the Roper Center. The advances in survey methods and in large-scale empirical social science are in major ways the remarkable accomplishment of organized social science. Without this infrastructure, funded in the early years by private philanthropy and more recently by the National Science Foundation, the steady, cumulative work that transformed, say, the Literary Digest Poll into the National Election Studies or local straw polls into the General Social Survey, would not have happened; nor, in that case, would the empirical social sciences as we know them today exist.

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# POLITICAL THEORY

**Modeling the World.** By Brian P. Bloomfield (New York: Basil Blackwell, 1986. xiv, 222 p. $49.95).

The objectives of this volume, in the author's words, are three (p. xi): (1) to elucidate the cultural tradition from which systems dynamics emerged; (2) to link intellectual style and content of systems dynamics to system dynamicists as a social group; and (3) to understand the exoteric role of systems dynamics in mediating and reinforcing external social structures.

In my words, the central objective is to show that systems dynamics is not a scientific method that is neutral in its sociopolitical (ideological) implications but that it is a fundamentally political (conservative) set of tools. For instance, the author states that systems dynamics "was orginally devised in the spirit of furthering American dominance—both economic and political—in the world system" (p. xi).

This "sociology of knowledge"—based critique has quite a lot in common with the critique of positivism made by critical theorists. In fact one of the most useful elements of this book is the sociology-of-knowledge discussion in chapter 2.

As someone who sees systems dynamics as a methodology that can be pressed into service by very ideologically diverse scholars, I have trouble with the argument. To develop it, the author focuses almost exclusively on the urban dynamics applications of the method by Forrester and the world dynamics applications by Forrester (World 2) and then the Donella Meadows group (World 3, or the *Limits to*

*Growth* [New York: Universe, 1974] model). There is no question that there are quite identifiable political-economy arguments developed in both applications (especially explicit in the Forrester work). But it is less clear that the political arguments are inherent in systems dynamics as opposed to reflecting the world views of some who apply it; and it is in making this distinction that the book fails. For example, the basic structure and behavior of the Bariloche or Latin American project model could be reproduced in a systems dynamic model. But that project was fundamentally critical of the *assumptions* and *prescriptions* (not the basic methodology) of the *Limits to Growth* project. Many methodologies are used by scholars with very diverse cultural, sociological, and ideological backgrounds. For instance, input-output matrices are used around the world and linear programming was simultaneously developed in the United States and the USSR (although initially rejected in the latter).

To prove that there is an *inherent* link of a methodology to a world view, one cannot rely on an analysis of the world views of those who initially use it (or reject it). Instead one must make a case linking basic features of the methodology with the world view. Bloomfield does not do this. There is minimal elucidation of the methodology—a single three-variable flow diagram of a heating system is presented, with little discussion. This illustrates a general and striking failure to penetrate a methodology that centers on causal loop and flow diagrams.

Instead, the author develops two principal (and a few lesser) lines of argument. The first compares Parsons and structural functionalism with Forrester and systems dynamics. The argument in brief is that (1) there is a set of common theoretical elements (including a systemic view of the world and an emphasis on interdependence) in the two approaches; (2) Forrester and Parsons share common characteristics (e.g., affiliation with elite educational institutions and elaboration of their basic approaches at about the same point in time); and (3) Parson's work is a product of his social development (relying on other work to make this case). We are asked to jump to the author's own conclusion, that systems dynamics is a product of Forrester's social development and that the methodology is fundamentally tied to that social background. This is roughly comparable to pointing out that the lives of Newton and Smith overlapped, that they both had global-integrative interests and that Smith was clearly a social product of Britain's industrial revolution and growing world role. Therefore, calculus must be tied to those same social perspectives.

Second, the author compares the "social experience" of the MIT Systems Dynamics Group with the Science Policy Research Unit at Sussex. He reviews the different nature of the institutions (e.g., exclusive MIT versus less-selective Sussex). He argues that there are fundamental methodology and thought-style differences in the two groups, focusing especially on criticisms by the Sussex group of the behavior and structure of World 3. This line of argument does not even prove that the Sussex group is opposed to the use of systems dynamics, much less that the methodology is tied to the "social experience" of the MIT group. In fact, in the first chapter of the critique prepared by the Sussex group, it is stated, "we argue . . . that the social sciences can benefit from the use of computer model-building techniques and specifically from systems dynamics" (H.S.D. Cole, et al. eds., *Models of Doom* [New York: Universe Books, 1973], 6).

Arguments that I find highly objectionable are developed in chapter 9 and, to be fair, the author does characterize them in the concluding chapter as "most speculative" (p. 197). In chapter 9 he draws an analogy between astrology and computer model-generated forecasts, arguing that they both have the characteristic of abstract authorities for most people (p. 176). What makes this especially upsetting is his developing this analogy after extensively analyzing a critique by the Sussex group of the MIT project based on Sussex's use of the MIT group's fully documented (equations, data, etc.) and openly distributed model. Perhaps the author would defend himself by falling back on the "for most people" qualification. But there are scholarly standards of replicability and proof. Newton and Einstein are also unquestionable authorities "for most people," but few scholars would attack their physics by comparing it with astrology.

In summary, the author makes his case for a social context and inherent world view in the models of Forrester and (less convincingly) others in the MIT Systems Dynamics Group. His attempt to extend this argument to systems dynamics fails.

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**Interpreting the World: Kant's Philosophy of History and Politics.** By William James Booth (Toronto: University of Toronto Press, 1986. xix, 189 p. $27.50).

In the celebrated and beautiful close of the *Critique of Practical Reason*, later abbreviated on his own tombstone ("the starry heavens above me, the moral law within me") Kant had stressed the gulf that divides nature and persons, the chasm that separates causality and moral self-determination; but in the *Critique of Judgment* he had striven to bridge that gulf through a "teleological substrate": persons freely pursue both "relative" ends given by "pathology" and reason-ordained "objective ends" (e.g., respect for persons as ends-in-themselves), while nature is interpreted through ends (purposes, functions) that mechanical causality cannot explain. He went on (in the "aesthetic" half of the *Judgment)* to propose two ways of "reading" the world: the "beautiful" (orderly purpose) and the "sublime" (awe-inspiring chaos).

James Booth uses the terminology of the *Judgment (beautiful/sublime)* to "reconstruct" (p. xvii) Kant's moral and political thought. He rejects the consoling, Christian, "beautiful" world (evidence of benevolent divine purpose, human evolution toward the "sweet dream" of eternal peace) in favor of a "Stoic," courageous confrontation with a sublime, but meaningless and terrifying, nature that we oppose bravely. "The Christian and the Kantian Stoic viewpoints, the beautiful and the sublime, are the great aesthetic views of the world" (p. 73). For Booth the "Kantian Stoic" viewpoint—which spurns a "romance" or "fiction written about nature"—is also the view of both Spinoza (who bravely perseveres while knowing the world to be an endless abyss, "one wide grave") and of Job, who rejects "comfort and consolation" in favor of "the sublime view of the world, of the rectitude without hope that Spinoza displayed" (p. 120). For Booth, then, Job, the Stoics, Spinoza, and Kant offer the same imperative: "to cling to duty, to be courageous . . . [to] look unflinchingly into the void."

Certainly this is a possible reading of Kant; and Booth's command of Kant's texts is so magisterial that this interpretation emerges with compelling force. But one should be clear what this reading costs, and what it loses. What Booth has done is to use the language of the *Judgment (beautiful/sublime)* to support the ideas of the *Practical Reason* (with its gulf between the "starry heaven" and the "moral law"). But the *Judgment* has as its main objective the bridging of that gulf: it urges *(Einleitung*, p. ix) that nature and morality may be linked through "ends"—nature is "read" purposively by us ("reflective" judgment), while morality has "objective ends" that we "ought to have" (the kingdom of ends, eternal peace). To reread Kant through the *Judgment*, to magnify beauty versus sublimity while suppressing the "teleological substrate" that unites morality and nature in "architectonic units", to reestablish the chasm between the stars and the self that Kant had arguably left behind when passing from the second *Critique* to the third—that is at best questionable. And the slighting of "ends" of all kinds leads to a misreading of Kant's central political idea. In the *Rechtslehre* Kant urges that eternal peace is the "entire ultimate end" *[Endzweck]* of justice. Since Booth rejects order and purpose and ends in favor of Stoic bravery, he is forced to see the ultimate ideal of Kantian politics as a "sweet dream" resembling "the poets' childish longing for a 'golden age'" (p. 114).

Then there is the problem of "Kantian Stoicism" itself; it is surely (at least) odd to view Kant as a Konigsbergian Epictetus when Kant consistently complained that the Stoics merely "called out reason instead of nature," that they had no notion at all of will. That is a serious lack, since Kant argues that a good will is "the only unqualifiedly good thing on earth"; and it is good will's absence that necessitates politics' presence. If all had the good will they ought to have, the kingdom of ends (automatically eternally peaceful) would obviate the need of kingdoms. (This is also a good reason not to identify Kant with Spinoza: if the Stoics merely lacked a notion of will, Spinoza positively declared will to be illusory, a fiction.) Certainly, Kant shares with Spinoza and the Stoics a devotion to duty; but since for Kant duty means acting from respect for objective ends (respect for persons *as* ends), it is ill advised to say that "the heart of the moral law is its non-teleological character" (p. 43). Kantianism is not a matter of "deontology" versus teleology, but of subordinating "relative" ends to "objective" ones.

PATRICK RILEY

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**The Redefinition of Conservatism: Politics and Doctrine.** By Charles Covell (New York: St. Martin's, 1985. xii, 267 p. $27.50).

This is an excellent analysis of the philosophical issues confronting conservative theorists in Great Britain. Charles Covell examines the intellectual underpinnings of the New Conservatism associated with the *Salisbury Review* by explicating the ideas of John Casey, Maurice Cowling, Shirley Robin Letwin, Michael Oakeshott, and Roger Scruton.

Three well-documented conclusions emerge from this study. First, these conservative theorists share a common critique of Cartesian rationalism, which leads to their defense of a number of traditional conservative values. Second, while these thinkers agree upon the name of the enemy, there is considerable disagreement about just which principles should constitute the bases of a conservative political philosophy. Indeed, Covell demonstrates that these conservatives owe much to the liberalism they attack. Third, it is often unclear how one gets from some of their general principles to the specific political programs advanced by conservative political leaders. Thatcherism can be defended or condemned, depending upon which conservative principles one places first and how one implements them.

It is not surprising that these conservatives attack the tradition of liberal rationalism associated with Descartes. What is surprising, and complicates their theoretical framework, is that their attack owes more to Wittgenstein and Hegel than to Burke. Covell points out that Casey and Scruton try to combine Wittgenstein's analysis of language and Hegel's historicism in rejecting liberal rationalism and in arguing that cultural and moral values are rooted in and justified by the social practices and language of a specific historical period.

While these thinkers illuminate the shortcomings of liberalism, there is no consensus among them as to what should be put in its place. The historicist arguments used by Casey and Scruton against rationalism call into question the natural-law tradition embraced by more familiar conservative figures such as Burke. Covell argues that their defense of "high culture," traditional family values, and virtues such as courage lacks an agreed-upon philosophical foundation. In fact, a number of these thinkers owe much more to liberalism than might be imagined. This can be seen in Oakeshott's debt to the Hobbesian view of the state as an instrument distinct from society and in Letwin's use of Hayek's economic ideas. While agreeing that socialism is not desirable, they do not agree in their assessment of Thatcherism. However, disagreements over matters ranging from Churchill's legacy to the Conservative party to contemporary economic issues are not fatal, nor are they unique to this movement. Liberalism and Marxism, too, contain many internal differences over policy questions.

As Covell explains, the real problem involves clarifying the basic principles of conservatism. Scruton, Oakeshott, and Letwin value constitutionalism, the rule of law, and procedural justice so highly that they give these norms the moral status of natural law. Yet they want these political ideas to stand as ultimate truths without developing a substantive theory of natural law that would justify such claims. However, the historicist arguments used against liberalism can be applied to conservatives' own notions of formal natural justice. Their thinking about law, social customs, morality, and the state is split between elements that are circumstantial and relativistic in tone and elements that are universalist and rationalistic in flavor. It is unclear whether the most important political ideas of the New Conservatism are to be justified in terms of the culture of a given historical period or in terms of truths that transcend any given social order.

This is a sophisticated study of an important group of conservative thinkers. It could have been stronger had Covell done more in measuring these thinkers against the conservative tradition established by Burke.

WILLIAM R. HARBOUR

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**Meditations on Modern Political Thought: Masculine/Feminine Themes from Luther to Arendt.** By Jean Bethke Elshtain (New York: Praeger, 1986. vii, 128 p. $32.95, cloth; $12.95, paper).

Jean Bethke Elshtain has long been known to political theorists for her insightful contributions to our understanding of the role of gender in the tradition of western political thought. In this, her latest work, Elshtain provides a provocative and concise resource that will be extremely useful to anyone seeking to explore and answer many of the most significant questions in contemporary political thought. In contrast to her *Public Man, Private Woman* (Princeton: Princeton University Press, 1981), which concentrated on the ways political theory devalued the private sphere, and consequently the women in it, this book directly confronts and analyzes the themes of masculinity and femininity as they have been portrayed in political theory. Elshtain suggests that political theory is itself partly responsible for the construction of gender.

Upon examining theorists as varied as Luther, Rousseau, Kant, Hegel, and Wollstonecraft, Elshtain finds that the history of political thought reflects changes in the importance and strength of the idea of femininity in relation to the masculine, more domineering impulse. She is less concerned with the explicit views of theorists about women than with the impact gendered themes and metaphors have upon our understanding of the female.

Elshtain contends that ultimately, many of the political theories examined are ambiguous on the question of gender. For example, Luther shifted the attention of protestants away from the Virgin Mary and toward Christ. His thought therefore represents a shift in favor of a masculine discourse insofar as he "eclipses" (pp. 11, 16) an important feminine symbol and replaces it with a masculine one (p. 17). In this way, individual women were liberated from the burden of comparison with the impossible ideal of the *virgin mother,* but the shift from Catholic to Lutheran thought also represents the decline of a major feminine metaphor in western political theory (p.18). Overall, Elshtain's focus on the symbol of *mother* rightly reflects the fact that the fate of *woman* has been linked to that of *mother.* But Elshtain seems to approve of this linkage despite its historical connection to female oppression. She praises, for example, Arendt's "natalist imagery" as one of "hope" (p. 110).

Rousseau, usually interpreted by feminists as an arch misogynist, is treated somewhat sympathetically by Elshtain who argues that his Emile and Sophie portray two realms of "sexual interdependence" (p. 39). Although Rousseau does argue, she continues, that female power is different from the male, he does not necessarily mean to claim that male power is superior (p. 39). Elshtain defends Rousseau against the familiar charges of sexism made by first-wave feminist Mary Wollstonecraft (pp. 40, 44, 47). She reminds us that although Rousseau does not consider women citizens, he preserves the feminine in that "feelings" (p. 44) and "Motherhood" (p. 49) figure prominently in his theory. Still, Elshtain's interpretation is not a wholesale defense of Rousseau; she agrees with Wollstonecraft, for example, that he "misidentified virtue" (p. 51) by linking it to military honor. Other first-wave feminist theorists are taken up later in Elshtain's discussion of "liberalism and its discontents" (p. 55). There, she provides a clear explanation of the early equal-rights tradition while continuing to explore the themes of femininity and masculinity.

Some readers will be disappointed by Elshtain's treatment of Hegel, whom she likens to Jane Addams. Both, she claims, realize that the "self cannot define itself outside complex social relations" (p. 77); both are "state enthusiasts" (pp. 75 and 80); and both specifically address male/female differences as they relate to the family (pp. 74, 78). Although these points are common to Hegel and Addams, there are also profound differences between the two. Addams, for example, begins her analysis by using "empathy and experience" (p. 77), categories that Hegel would dismiss as unmediated and abstract. Although Elshtain does not find these differences important, Hegel scholars might disagree.

By calling her book a "meditation," Elshtain underscores her intention to engage in an "act of continued reflection" (p. xi). One, therefore, expects the work to raise questions as well as answer them. And it does. The account of Luther, for example, forces us to consider the trade-off between maintaining feminine symbols that privilege women even as they oppress them and creating a masculine (or even a gender-neutral) discourse that ignores those symbols. Similarly, Elshtain's implication that there is a positive link between *woman* and *the feminine, woman* and *mother,* leads us to ask how we should treat the relationship between the biological capacity to mother and the social practice of motherhood. In sum, Elshtain continues to explore new terrain in contemporary political thought, asking as well as answering vital questions for feminist theory.

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**To Breathe Freely: Risk, Consent and Air.** Edited by Mary Gibson (Totawa, NJ: Rowman and Allanheld, 1985, xv, 294 p. $35.00).

This interdisciplinary book about obstacles to unpolluted air is in three parts, together with an epilogue. In Part 1, "Risks of Airborne Contaminants," J. Repace argues that smokers have no right to endanger nonsmokers. L. Punnett discusses cancer hazards in the workplace. R. Friedman discusses risks from transported air pollutants. In Part 2, "The Meaning and Role of Consent," S. Scheffler distinguishes three views on consent—instrumental, intrinsic, and rights-based—and argues for the first. P. Railton exposes a traditional Lockean property notion as being inadequate to account for needed restraints against pollutants; and argues that Locke instead held a common property notion that counts against the idea of exclusive individual property. J. J. Thomson examines the meaning of consent in troubling air pollution problems. She raises questions of the usefulness of the idea of consent, which she illustrates as follows: If one knows that working in a factory or that living in a certain neighborhood or that walking down "Unpleasant Way" is risky, doing so implies consent (p. 140), but to say there is consent "goes no way . . . toward removing my ground for complaint" about what happens in the case of the factory or the neighborhood. M. Gibson argues that respect for individual autonomy underlies consent and applies her thesis to second-hand cigarette smoke, workplace air pollution, and outdoor air pollution. To Gibson, worker autonomy, in particular, means the right to know workplace hazards, risks, potential toxins; the right to refuse to take on a work assignment in an unsafe place, without penalty; and the right to participate in work-related and national health and safety policies. B. Fishhoff marks off some problems with informed consent, what it implies, and its limits in minimizing unwanted hazards. M. Baram argues that consent is a vital aspect of the common law for managing health and safety risks.

In Part 3, "Pollution and Policy," M. McCarthy writes on occupational safety. He exposes noise pollution and silica in abrasive blasting operations. He urges institutional reforms to cope with inadequate controls of workplace hazards (p. 202). G. Eads notes that a rights view conflicts with trade-offs and prefers the first of these views. C. Russell asks whether a cost-benefit policy on deciding what forms of pollution to regulate is ethically defensible or politically meaningful. He proposes five criteria for evaluating the implementation of air pollution regulations: efficiency, informational intensity, monitoring enforcement, incentives to polluters, and political and ethical appeal (p. 238). He applies his proposed standards to three areas: outdoor air, workplace, and side stream cigarette smoke. He raises questions about the difficulties in effective monitoring devices. Finally, in the Epilogue, "Hazards of Entering the Risk Debate," L. Winner writes that a politically conservative bias is inherent in the assumptions of risk management and that the language of risks misdescribes health-pollution problems. Winner notes two assumptions, that "hazards to health and safety are fairly easily known" (p. 273) and that "when hazards are known, agreement on what to do is readily available among . . . reasonable people" (p. 273).

This book is packed with conceptually penetrating essays on consent and risks in relation to obstacles to clean air (e.g., Railton, Scheffler, Thomson, Gibson). It is also stacked with data, charts, and threshold problems of airborne substances. Issues about the limits of individual consent to the quality of air pervade this book. Yet, there is not much interplay between these independently fine essays. Only Gibson considers issues raised mainly by her fellow philosophers. Otherwise, there is not much by way of orchestration. Also, to the question how to regulate relatively unknown air pollutants, this book provides little.

Puzzling also is why *consent,* a generally important, but defeasible political concept, commands center stage in this book. Risk, consent, and air presuppose a far more fundamental concept, namely, *harm.* Why discuss risk and consent if neither is related to harm? One or two thought experiments may help. Imagine that Jones has a choice to enter either of two rooms for an hour. There is an equal chance to emerge from either room with great benefits. One room gives Jones the right to consent before anything is done, but also (as Jones is truthfully told) involves a risk of harm. A second room gives Jones no right to consent before anything is done, but involves no risk of harm. Jones will choose to forego consent.

One may recall M. Walzer's famous example of the magical pill put into the community drinking water with the people's knowledge, that instantly cured those who drank of it of sexism and racism. Imagine that we had a pill that, if placed in the water supply, would instantly give those who drank of it a wise and sensitive drive against nuclear abuses; but again it would be without the consent of these people. Imagine also that there *is no other way* to restrain people's drive toward eventual nuclear annihilation. In those circumstances, what is one to say of the precious virtue of consent? Consent is ordinarily a great value, but it is not the only means of restraining a known or unknown harm. Fortunately, in our actual world, the only kind of pill we have is based on knowledge, and its development and use implies consent. As Chernobyl reminded us, we can't afford to know too little, if we are to survive under decent conditions.

I would add some distinctions to those the book makes: Potential harm may be (1) on a small scale, such as individual cigarette smoking; (2) on a medium scale (e.g., noxious chimney and related industrial pollutants); and (3) on a large scale (e.g., nuclear plants). One may argue that by accretion of the small-scale pollutants, harm comes on a large scale. But then, one could be tagged with the Heap fallacy. The hazards of smoking, undoubtedly serious to its victims, are on a small scale in relation to the large-scale effects of inadequately controlled nuclear energy.

The book performs a service in reminding us how important it is for us to breathe clean air. But it falls short by not telling us that carrying on life on earth with the use of nuclear energy is a high-risk, fragile, precarious enterprise. The inability or unconcern of nation states to stake boundary posts around their own emissions leaves serious problems not only for free air and for freedom but for control as well. As there is a rights-duty correlativity, so there is a freedom-control correlativity. One can't have freedom without control.

The beneficial uses of science and technology also raise some other critical questions. Will we Earth inhabitants settle for no-risk, moderate-risk or high-risk technology? Sinclair Lewis raised a similar question in *Arrowsmith.* Dr. Gottlieb wants to make sure about a drug to combat a plague. Dr. Arrowsmith is willing to take a bigger risk to arrest the disease that threatens the entire community.

Imagine a hypothetical group called Smithlanders, who initially give consent to benefits and some risk of harm in having a nuclear reactor. Then, in the building process, the ante goes up in risk of unwanted effects. The question is, How do Smithlanders later withdraw from giving consent *simpliciter* to undesirable and unexpected aftereffects that they can no longer prevent? We inherit the conditions of the earth that our predecessors left us.

Imagine, finally, that at one time Smithlanders consented to the use of nuclear power, aware of risks, but confident that God or nature was on their side, or at least that, logically, there couldn't be a meltdown. But then one day, there was a serious nuclear explosion somewhere in Smithland, and then one afternoon, the earth went up in a puff of smoke. What good would previously given informed consent be? This otherwise promising book does not come to grips with this set of questions.

BERTRAM BANDMAN

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**The Captive Public: How Mass Opinion Promotes State Power.** By Benjamin Ginsberg (New York: Basic Books, 1986. xi, 232 p. $18.95).

In reference to Benjamin Ginsberg's *The Captive Public: How Mass Opinion Promotes State Power,* we bring both good news and bad news. The good news is that we are given an often brilliant, provocative, and creative analysis of modern public opinion. This is a landmark book whose influence should be felt well beyond our profession. The bad news is that it is unlikely to generate the type of discussion and research that the study of public opinion sorely needs. If there is a list called "Great Books to Be Ignored by Political Scientists," *The Captive Public* is an excellent candidate for it.

Ginsberg's basic theme is that the emergence of modern public opinion, that is, the results of survey research, greatly strengthens the state. This view of mass opinion is profoundly different from the one held by most contemporary researchers. It challenges the almost sacred assumption that the revelation of public thinking is primarily a force for greater citizen control of government. In certain fundamental ways, opinion polls in a democracy are no different from those in a totalitarian state: both are part of an elaborate system of controlling a potentially disruptive population.

Chapter 3, "Polling and the Transformation of Public Opinion," is a particularly lucid and insightful explication of this theme. In it, Ginsberg makes four points regarding the emergence of modern survey techniques. First, thanks to polling, individuals are spared the burden of having to volunteer their views. This has major political ramifications—intense and extreme opinion is likely to be overwhelmed in a sea of weak and nonexistent opinion. The fanatic can, with scientific precision, be easily dismissed as an oddball. Second, potential danger is now revealed in the form of attitudes, not behavior. The government can "solve" riots long before they occur by analyzing survey results and implementing appropriate public relations campaigns. Third, polls allow dominant interests to bypass group leaders as public spokespersons. In particular, polls are likely to show that spokespersons of the Left do not lead citizens uniformly sharing a leftist ideology. Finally, the existence of polling gives those who control polling an enormous influence over the public agenda. Race relations may be a pressing concern to millions of blacks, but this issue may be "crowded out" by questions on flying saucers and beliefs about God. In sum, as Ginsberg notes, the modern opinion poll transforms—"public opinion from an unpredictable, extreme, and often dangerous force into a more docile expression of public sentiment."

Equally provocative is Ginsberg's analysis of the "marketplace of ideas." Contrary to what is claimed in innumerable commencement speeches, this marketplace is not some eternal and politically neutral arrangement in which the weak and the strong compete as equals. To begin with, such a marketplace must be created by government—the historic "natural" state of affairs is group isolation and near impossibility of political persuasion. Modern governments have used several techniques to construct this marketplace. The imposition of a common language and mass literacy permits all segments of society to participate. Moreover, universal state-run schooling created shared perceptions of the world—every American knows of the Pilgrims, the Founding Fathers, and so on. Mass communications mechanisms, from roads and railroads to radio and TV, provided the channels by which people could talk to each other. Finally, there was the emergence of protected free speech as a legal doctrine.

Does all of this add up to some sort of intellectual equality? Definitely no, argues Ginsberg. The marketplace of ideas in political reality permits the strong to dominate the weak more effectively. Prior to the marketplace, the weak may have been dominated physically, but intellectual and cultural domination was impossible. Today, domination is more complete. A poor person must not only contend with superior physical force, but he or she may also have absorbed a view of the world that legitimizes this subordination.

These summaries merely scratch the surface. Other chapters deal with elections, the impact of technology on shifts in political power, the significance of these findings for democratic theory, and other opinion-related topics.

Now for the bad news. Why is it that this book will have only a modest impact on the field? It is obvious to anyone who reviews the study of public opinion among political scientists that "normal" research emphasizes the milling of certain generally available data sets. Individual attitudes/opinions are the unit of analysis; the object is to explain distributions using (largely) other individual-level factors. This approach is reflected in the various controversies regularly surfacing in the journals: the existence of ideology, political trust, tolerance, the character of partisan identification, and so on. At its core, the study of public opinion asks: why do people hold particular views?

Ginsberg, however, sees public opinion as an integral part of the political process. Polls exist because they are politically useful; they are weapons. Survey data are important because they help illuminate the results of the struggle. The core question here is, what is the political impact of public opinion? (The answer, for Ginsberg, is *greater state control.*)

Surely both approaches are valid. Unfortunately, few incentives exist to pursue the second approach. Imagine a young scholar contemplating a career dealing with public opinion. He or she is no doubt trained in statistics, knows how to use Statistical Package for the Social Sciences (SPSS) and is familiar with several existing data sets. There are also strong pressures to be rigorous, display a deep concern for methodological correctness, and address the traditional questions of opinion research. One can almost see papers called "A Dynamic Analysis of Political Trust: The Case for a Multivariate . . ." blazing forth of their own volition.

From this young scholar's viewpoint, *The Captive Public* appears "soft," impressionistic, unscientific, and distressingly unconcerned with matters methodological. Hardly an approach that will gain professional fame and fortune. It is also a difficult approach to execute—there are few textbook guidelines on how to ask fresh and provocative questions. For these reasons, *The Captive Public* will probably have little impact "other than" to offer some major insight on the subject of public opinion and political power. Given the current state of public opinion research, this is indeed very bad news.

ROBERT WEISSBERG

*University of Illinois, Urbana-Champaign*

**Delusion: Internal Dimensions of Political Life.** By James M. Glass (Chicago: The University of Chicago Press, 1985. xxiv, 270 p. $22.50).

This is a book with a mission and a target audience. Political theorists are the target audience for whom Glass seeks to demonstrate that their thinking about politics and people would greatly benefit if it were to become informed by knowledge about the inner world of people. "In my view, political theory should be an activity that focuses on the relationship between the self (particularly its unconscious structures) and political life" (p. 247).

To make and "prove" this point Glass relies on his observations and interviews with institutionalized delusional schizophrenics—an inquiry he carried out over a six-year period, mostly at Sheppard and Enoch Pratt Hospital in Baltimore. It is his contention that the "internal image of the schizophrenic can reveal to the social world tendencies that explain basic facts of human nature. In this sense, delusional language helps define just what 'human nature' is" (p. 66). At the core of these internal images, according to Glass, are power relationships, with the patient oscillating between feelings of victim and victimizer, between feelings of utter helplessness and omnipotence. Problems of authority relationships (of dominance and subjugation) are as crucial for the schizophrenic as they are for men in general.

Glass describes vividly and sympathetically the world of the paranoid. It is a world dominated by immobilizing, all-encompassing fear which makes human communication and cooperation all but impossible. He does an excellent job in demonstrating the inner logic that organizes this world. He is most convincing when he exhorts us to treat their "waking nightmare" as a real experience with an inner reality all its own. This exhortation, though not novel, is well taken. Considerably more questionable is whether we are permitted to draw the parallel he draws between the paranoid's delusional world and the world of the so-called normal person.

It is equally hard to see the relevance that Glass sees of the schizophrenic experience to the world of politics. Glass may well be correct that the "delusional utterances are as real and substantial as any other set of relations" (p.

137), but inasmuch as they are delusional and remain locked within the self of the patient (until "unlocked" by the therapist) what practical applicability do they have to political thought and practice? Glass, of course, would answer (quoting the Finnish psychiatrist Siirala) that the prophesies of the delusional self "are the insights into our collective sickness." The jury is still out on the diagnosis.

The parallels he draws between the delusional world of the schizophrenic on the one hand and the world of the political dictatorship are misleading and disturbing, especially when he suggests that the values of political tyranny might be "similar to those of schizophrenic delusion," and that images "of sadistic tormentors, which are persistent in the delusional drama, may have a great deal in common, in terms of their affective origin, with political types whose ascendance depends on domination and torment" (p. 39). Comparisons such as these come uncomfortably close to "psychologizing" the all-too-real and rational character of modern dictatorships.

Glass is considerably more convincing when he argues that political theorists should focus more than they are apt to do on human nature and develop a concept of human nature informed by the insights psychology and psychiatry can furnish. "Why," he asks, "are current political theorists reluctant to look at methodologies, terminologies, and perspective directed toward understanding and analyzing internal psychological states?" (p. 249). Given that politics and people are inseparable, that is a good question indeed.

ROBERTA S. SIGEL

*Rutgers University*

**Alexis de Tocqueville: Threats to Freedom in Democracy.** By Michael Hereth, translated by George F. Bogardus (Durham, NC: Duke University Press, 1986. vii, 195 p. $27.50).

Michael Hereth has written a contentious book. He attempts not only to give a new account of Tocquevillian political theory, but to situate Tocqueville in the tradition of political rhetoric and to show the limits of this tradition. The book is divided into four parts. The first two parts, which make up the bulk, address Tocquevillian theory and Tocquevillian practice respectively. The two concluding parts ("Algeria" and "Liberalism") present a critique of the relation between theory and practice as it emerges in Tocquevillian rhetoric and statecraft.

Part 1 ("Freedom in the Republic") is, in general, a solid introduction to Tocquevillian political theory. Hereth lays particular emphasis on the republican aspects of Tocqueville's work and the urgent necessity of creating a civic ethos compatible with democratic social forms. For Hereth, Tocqueville's perennial concern was whether and under what conditions disorder might be subdued by the opinions, sentiments, and habits of citizens. Hereth rightly sees that Tocquevillian liberalism thrusts the *political* into the center of the free way of life, that this way of life is an acquired taste, and that its maintenance requires an experiential knowledge and practical political skill founded on habit and judgment. This emphasis on practice is appropriate, but Hereth's presentation is unrelenting, often repetitive, and sometimes tiresome. To these difficulties is added an occasionally wooden syntax that is particularly unfortunate in a book that takes rhetoric so seriously. Moreover, while Hereth recognizes the republican pedigree of Tocqueville's view of freedom, he reads Tocqueville through an Arendtian prism where freedom becomes simply the freedom of citizens *to act and perform.* As such, Hereth obscures Tocqueville's understanding of political authority and its relation to freedom. Nonetheless, this is a rich and textured account.

Part 2 ("Alexis de Tocqueville: The Political Man") breaks new ground. Throughout, Hereth makes good and frequent use of Tocqueville's letters and speeches in addition to his published work. For Hereth, these materials as well as Tocqueville's *Recollections* do not simply shed light on Tocqueville's theoretical reflections, they are the remnants of political action. Tocqueville's choice of a political career over a life of contemplation is of premier significance. Hereth calls attention to the tension in Tocqueville between persuading and understanding, and to Tocqueville's extraordinary sensitivity to audience. These insights are important contributions to Tocqueville scholarship.

Parts 3 and 4, in which the limitations of

Tocqueville's practical philosophy are addressed, are both the most ambitious and the least satisfying. Hereth's critique rests upon a distinction between the tradition of political philosophy and that of political rhetoric. Those in the former tradition go unnamed. Tocqueville's predecessors in the latter tradition include Isocrates, Cicero, Petrarch, Salutati, Bruni, Machiavelli, Montesquieu, Burke, and Madison. The parameters of these two traditions are presented only in outline. The philosophical tradition "puts ability to perceive and human reason at the center of virtues" whereas rhetoric trains "a citizen versed in public affairs . . . to 'teach, converse, but especially to move and convince' his fellow citizens." The former tradition aspires to "truth"; the latter remains within the realm of "belief" or "opinion." The end of philosophy is the "good life," that of rhetoric "power" or making real one's identity as a "speaking being." As Hereth sees it, the rhetorical tradition is closely connected to republican forms, that is to "debate by the free and equal." Whether the philosophical tradition is likewise linked to a regime type is not discussed (although Hereth mentions Plato in passing). The central problem with the rhetorical tradition (which must necessarily remain within the boundaries of community opinion) is that it leaves undebated the "problem of good and just government" (pp. 92-93).

Tocqueville, we are told, found himself torn between "the desire for certainty," his "aloofness from prevailing interpretations of political order," and his efforts "to influence the opinions of his compatriots" (p. 89). But Tocqueville had fallen "victim to the reduction of reason to pragmatic rationalism," which "discredited reason as such" (p. 163). Despairing of reasonable order, he embraced a politics of passion and sought to arouse opinions capable of resisting calculating self-interest. Like other nineteenth century Europeans, convinced of the superiority of European civilization and seeking a basis for the heroic passions, Tocqueville sought "to transform this superiority into dominion" (p. 133). Tocquevillian political theory, unable to break through the limits of opinion within which the rhetorical method is imprisoned (on two occasions, we are told of Tocqueville's failure to "pursue his train of thought to its conclusion" [p. 143]), must bear its full share of responsibility for the crimes of French colonialism. "Why," asks Hereth, "did not Tocqueville apply his knowledge of the injustice and inhumanity of foreign rule to the Algerian situation?" (p. 159). Hereth's answer: the "vagueness" of Tocqueville's idea of freedom, and his "lack of knowledge of good order" prevented him from "advancing to the way of thinking of a man shaped by reason and its openness" (pp. 162-63).

I am not certain how to respond to all of this. I could say that Hereth has fundamentally misunderstood the relation of reason and the passions in Tocqueville. I could say that the distinction between the traditions of political philosophy and political rhetoric is incoherent (*all* political philosophy exists within the tension between persuasion and truth, opinion and knowledge). I suspect, however, that such objections would not suffice. Michael Hereth would still lament that a free way of life does not "automatically motivate citizens to strive for justice" (p. 169), or that republics fail to "mold" the minds of their citizens "by objectivity toward experience and love of truth" (p. 170). Politics is not without its inconveniences.

Bruce James Smith

*Allegheny College*

**Politics and Paradigms: Changing Theories of Change in Social Science.** By Andrew C. Janos (Stanford, CA: Stanford University Press, 1986. ix, 183 p. $28.50, cloth; $10.95, paper).

Janos is a specialist in East European comparative politics with primary interest in Hungary. This book records his interpretation of two major changes, or "paradigm shifts," in the field of comparative politics in the last decade. Or rather, it reports the changes in his own thinking since 1976.

In 1971 Janos's thinking was based mainly on Huntington, Apter, Pye, and Deutsch (Janos and William B. Slottman, eds., *Revolution in Perspective: Essays on the Hungarian Soviet Republic of 1919* [Berkeley: University of California Press, 1971]). The basic idea was that the problem of modernization for Hungary in 1867-1918 was analogous to the situation in the emerging nations, not to the situation in the advanced capitalist countries.

The elites in the emerging nations had to set up "mobilization systems," rallying the masses around utopian visions and repressing dissenters, in order to produce the rapid development needed for the takeoff to self-sustained growth. In 1976 he systematized this elitist theory of rapid development, listing seven variables and four types of political regimes in Eastern Europe (Janos, ed., *Authoritarian Politics in Communist Europe* [Berkeley: University of California Press, 1976], pp. 2-3, 7).

By 1982 he was in process of absorbing the Marxist world-system theory of Wallerstein, Gunder-Frank, Emmanuel, Amin, and Cardoso (*The Politics of Backwardness in Hungary, 1825-1945* [Berkeley: University of California Press, 1982], preface), a process which continues in the present work. The basic idea is that there is increasingly a global rather than a societal division of labor, and the evolution of this division of labor imposes at least nine structural disadvantages on peripheral nations, which cumulatively produce relative and absolute impoverishment. Consequently, even the most charismatic and fortunately placed modernizing elites can scarcely overcome the constraints of peripheralism, and may choose to work for revolutionary change of the world system rather than hopeless national development.

Janos briefly describes a second "paradigm shift," which asserts that in the postindustrial United States, the elite are losing their legitimacy, their grip on the masses (chap. 5). The clearest statements of this theme are Bell's 1976 lament that we need a new public philosophy because the masses are losing their faith in the old one and Brzezinski's 1970 references to counterrevolutionary students in the coming technetronic age. These comments were superficial attempts to explain the student Left and other recent radical movements, and one cannot call them a theory, let alone a paradigm. Janos supplements them with odd bits and pieces from here and there; he even has Habermas saying that elites may lose their communicative competence, their ability to manipulate mass fantasies. This is Habermas? Apparently Janos is constructing his own theory of "postindustrial" culture and has not gotten very far yet; but with the triumph of Ronald Reagan the theory is already obsolete.

Janos's account of world-system theory is much more thorough. He has read widely and understood much (chap. 3). But here, too, his thinking is still in process. The chapter provides a long catalogue of pieces of theory, but not a systematic theory. Apparently, Janos is working pieces of the new approach into his old way of thinking, rather than seeing world-system theory in its own terms. For him it becomes a list of the almost insuperable barriers facing modernizing elites in the peripheral countries. To be sure, the theory itself is fragmented and changing, but to see it as a theory of the problems of modernizing elites is to give it an odd slant.

The basic difficulty is that Janos insistently presses *his* questions on all these works, instead of asking what questions the writers were trying to answer. His questions are, Change is from what to what? Is that good or bad? What is the basic cause of change? The result is such absurdities as calling Parsons's pattern variables a theory of change (pp. 41-44). Indeed the whole treatment of Parsons is wierd. The treatment of dialectic is also superficial (p. 16).

Alternatively, perhaps Janos's writing style is too condensed to express his full thoughts. Throughout the book complex theories are simplified to a single doctrine in a single book, taken out of context and interpreted via far-fetched analogies. To call the Mannheim of 1940 in England a despairing pessimist (p. 128) is so far off that it suggests Janos was oversimplifying the earlier German edition; but to assert that Fromm and Arendt were carrying on Mannheim's "tradition" of pessimism is absurd. Nor do I understand the point of calling Habermas a Left Paretan (p. 144).

However, one must go beyond these superficial oddities to appreciate the book. This work is not at all a reliable account of social science theories, but it is a sustained attempt to understand the problems facing elites in East European and other peripheral countries.

PAUL DIESING

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**Nicos Poulantzas: Marxist Theory and Political Strategy.** By Bob Jessop (New York: St. Martin's, 1985. xviii, 391 p. $39.95, cloth; $14.95, paper).

Jessop's book is a sympathetic and convincing analysis of the development of Nicos Poulantzas's political thought that, paradoxically, is not likely to find much favor among those Marxists most influenced by Poulantzas.

A major figure in the resurgence of Marxist political theory during the 1970s, Poulantzas had enormous impact upon contemporary conceptions of the capitalist state through his difficult books and celebrated debate with Ralph Miliband. For many young Marxists, deeply disenchanted with the crude "theory" of orthodox Marxism-Leninism, Poulantzas seemed to offer a methodologically rigorous and comprehensive approach to the state in capitalist class society that recognized both the complexity and "relative autonomy" of the political sphere. At the same time other Marxists, disturbed by what they saw as a formalistic displacement of class struggle from politics, were increasingly obliged to confront the ideas and language of Poulantzas's analysis.

In the years since Poulantzas's untimely and tragic death, a new debate has emerged among theorists of the Left, between those who continue to insist on the primacy of class struggle and working class movements in making a transition to democratic socialism and those who reject such "class reductionism" and instead advocate broadly based "democratic struggle," especially involving "new social movements," as the way towards socialism. Jessop writes from this latter perspective, and argues that the central point of reference in the development of Poulantzas's political theory was his commitment to the politics of class alliance, which in the end leads towards just such a reformulation of the socialist project.

Jessop bases this claim on a close and systematic reading of Poulantzas's work, from student articles to unfinished research notes, taken in the context of his developing political strategy. He critically examines Poulantzas's Sartrean, *"existentialo-marxiste"* period; his Althusserian "regional theory of politics"; later theoretical reformulations influenced by Foucault; and the strategic/theoretical problems of his final work. This focus on Poulantzas's *development*—through the influence of French philosophy, Italian Marxism (della Volpe, Gramsci, and Ingrao), and his early training in Romano-German law—imposes a sometimes awkward structure on the book. Yet these diverse elements are handled clearly and without heavy reliance on Poulantzas's jargon.

In key chapters on "Social Classes and Class Alliances" and "The Democratic Transition to Democratic Socialism," Jessop explores the underlying connection between Poulantzas's conception of "relative autonomy" and his left-Eurocommunist political strategy. At issue is the relationship between what Jessop refers to as *class determination* and *class practice.* Jessop recognizes that Poulantzas from the start embraced the idea of relative autonomy in order to escape from the "economism" of orthodox Marxism-Leninism. He argues, however, that Poulantzas never resolved the theoretical problem of treating class determination in the economic sphere and class practices in the political sphere as *different* aspects of the *same* phenomenon—that political practice is autonomous, but only relatively.

Jessop convincingly establishes the priority of a strategic continuity in Poulantzas's theories, with his rigorous methodological insistence on the determinacy of class "in the last instance" serving largely as a Marxist anchor for an otherwise unfettered politicism. In Jessop's view, though Poulantzas moved away from Althusserian structuralism, towards a "relational" theory of the state, he finally failed in his effort to develop a consistent political theory corresponding to his strategy of class alliance because of his "reluctance to abandon what he believed were the essential principles of historical materialism, namely, economic determination in the last instance and the primacy of class struggle" (p. 146). Jessop argues that initially this was integral to Poulantzas's problematic structuralism, but subsequently, after self-criticism, "Poulantzas treated the articulation between class determination and class practice as contingent and conjunctural" (p. 188), and came to recognize the importance of "new social movements," making this "class reductionism" theoretically inconsistent.

Recognizing that Poulantzas himself never resolved the problem, Jessop alternates between sympathetic appreciation for the direction of his thought, and sharp criticism of its inconsistencies. Those who have questioned the usefulness of Poulantzas's theoretical formulations may be wryly amused by the extent of this criticism, which locates Poulantzas's real contribution in the trajectory away from class struggle. Marxists who accept Poulantzas's framework, but reject Jessop's political view, might well be discomforted.

Whatever one thinks of Jessop's views, the great weakness of the book is its failure to consider other criticisms of Poulantzas. Incredibly, Jessop excuses himself from any discussion of the Miliband debate—on grounds that it captured Poulantzas at his most structuralist, making it irrelevant to his work as a whole. Critics of Poulantzas's displacement of class struggle are simply ignored, although Ellen Meiksins Wood had earlier examined the link between Poulantzas's political strategy and his theories of class and state, and the implications of the political shift away from the working class ("Marxism without Class Struggle," *Socialist Register* [1983]). Despite its strengths, therefore, Jessop's book falls far short of being a definitive critical study of Poulantzas's contribution to Marxist political theory. Perhaps ironically, however, Jessop—with his injunction to move "Beyond Poulantzas"—helps to make clear just how untenable a midground "Poulantzasian Marxism" has become.

GEORGE C. COMNINEL

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**Economic Justice: Private Rights and Public Responsibilities.** Edited by Kenneth Kipnis and Diana T. Meyers (Totowa, NJ: Rowman and Allanheld, 1985. viii, 250 p. $28.50, cloth; $13.50, paper).

This volume collects a sample of papers from a 1983 University of Florida conference on social and legal philosophy. Conference collections usually display certain characteristic flaws. This volume is largely free of them. Fully a third of the 18 chapters are brief comments on the other papers, it is true. But most make points that strike deeply at the main thesis of the papers, points that the main authors would have had a hard time accommodating in their revisions. Some of the best moments in the book come in these exchanges. For once, the generality of the title does not mask any great disparity of themes. Virtually all the chapters concern economic justice, welfare rights, and cognate problems of social policy. Stylistically, virtually all operate in the mode of modern analytic philosophy; even Timo Airaksinen's Hegel (substantively, definitely one of the outliers in the set) gets analyzed very much in the mode of G. A. Cohen's Marx. Finally, and most importantly, the quality of the various chapters is surprisingly consistent. Virtually every chapter is perfectly sound, though few are exciting. All the basic points have been heard before, often (as with Gewirth's lead essay) from the very same authors.

Lack of originality makes this more a collection for libraries and students than for research scholars. But for teaching purposes, this is a very serviceable collection. Alan Gewirth's summary of how to derive welfare rights from his principle of generic consistency is delightfully paired with Virginia Held's sharp riposte, "What is the political power of pure reason?" Theodore Benditt's defense of justice as reciprocity evokes a nice reply from Bruce Landesman arguing for a more thoroughgoing egalitarianism; and the cognate question of where the needs of nonproducers fit into the Rawlsian framework is taken up again by Rex Martin. The basic logic of Henry Shue's important argument in *Basic Rights* (Princeton: Princeton University Press, 1980) is set out succinctly by James Nickel. There is little new in any of this, but the uninitiated would enjoy and profit from it all.

One of the exchanges in the book does strike me as particularly novel, though. The burden of Lawrence Becker's paper on "Property Rights and Social Justice" is that since justifications for property rights are largely based on considerations of social welfare in one way or another being served by such an institution, the selfsame social-welfare considerations that ground property rights themselves should be able to justify us in overriding those property rights from time to time to promote social welfare in other ways. Like can override like, morally speaking. Putting Becker's point even more pointedly, Diana Meyers distinguishes (in my words, not hers) between rights that are foundational and rights that are merely derivative. If foundational, rights cannot be overridden, except by other rights of broadly the same kind or status; and then we really would need a strong concept of "welfare rights" that assimilates them to "property rights" (in the manner of Reich's "new property," perhaps) to justify tax-transfer systems. If merely derivative, rights can be overridden on the selfsame grounds that they were instituted in the first place—here, on grounds of social welfare.

This is a fruitful reformulation of points that have heretofore been, at best, only half-appreciated. But, as often happens, a clear statement simply serves to bring clearly into view flaws in the argument. Here, it is the proposition that that which is derivative can simply be overridden by that from which it is derived. What that formulation crucially omits is history. That which is derivative *could have been* overridden by that from which it is derived, had we decided to do so in the very first instance, when first planning our social institutios. But, having established institutions of private property and allowed people to invest their energies and resources and, perhaps, personalities in them, a new principle comes into play; and it is in some deep sense unfair, and not merely counterproductive of social welfare, to take that property from them now—especially without compensation, as is done in tax-transfer systems. Of course, Becker is right to say that considerations of social welfare will usually be sufficiently strong to override such considerations of fairness, on any sensible scale of what matters most morally. But that reply is to miss the point. The beauty of Becker's initial strategy was to suggest that there could be no question about the legitimacy of trading like for like in overriding property rights in favor of social-welfare programs. The force of the fairness reply is that, whatever considerations may initially have guided us in establishing institutions of private property, once they have been established considerations of *another kind* come into play. It is no longer just a matter of trading like for like, and, that being so, those who want to argue for the incommensurability or strong priority of this new kind of consideration now have an opening for defending property in that way—an opening that Becker's initial strategy had hoped to deny them.

ROBERT E. GOODIN

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**The Development of Plato's Political Theory.** By George Klosko (New York: Methuen, 1986. xx, 263 p. $31.00, cloth; $11.50, paper).

While professedly "not intended primarily for specialists" Klosko's work is also more than an introduction. Specialists, too, will benefit. And while he claims to prefer "to be reliable rather than new," few students of Plato's political thought will come away from this work without seeing at least certain questions differently. It proposes a familiar idea—that there is an evolution in Plato's thought from early to late. The work is original and important because of the nature of the evolution Klosko discovers and how he demonstrates it.

At the center are Plato's conception of the human soul and the possibilities and strategies that exist for moral improvement. The view of the younger Plato (the Socrates of the "early" dialogues) was too simple, too one-sidedly intellectualistic. The soul is treated by Socrates in these dialogues as though it consisted of reason alone. Moral failure is regarded in these works as "a case of intellectual error" (p. 44), and moral correction becomes an academic exercise, an exercise in rational debate. Political consequences follow: avoid public life (one cannot debate a crowd) and attempt to improve others by strengthening through philosophical discourse the health and vigor of the rational faculty. To become good is to become knowledgeable; virtue is correct knowledge.

At some point, perhaps still while he was composing the earlier works, Plato came to see an error in the Socratic position. Not reason alone, but also passion and appetite constitute the human soul. Moral error is now traced not to a failure of knowledge but to a predominance of appetite and passion over reason. Political language is used to convey this idea; appetites rule, reason is enslaved. Given these new psychological views, "Plato must reject the idea that arguments alone can change characters" (p. 50). Since the rational faculties of most people are "held hostage by their desires . . . there is a definite limit to what arguments can do to help them" (p. 51). There are obviously important political implications for this new conception of the soul, and these are fully worked out in the *Republic*. Reasoned discourse still plays a role, but now within the context of "a tightly controlled city of people having virtue imposed upon them from without" (p. 129). The Socratic ideal of morally autonomous individuals gives way to a society in which people are instructed what to believe and how to behave; habituation largely takes the place of open inquiry. Only philosophers are any longer believed to be capable of genuine virtue, and "even in their case this is possible only because they too are subjected to rigorous conditioning in their youth."

Plato's view improves upon the Socratic, Klosko thinks, by recognizing the complexity of the human soul and so allowing for genuine psychological conflict. It also entails a more exalted conception of human knowledge, based as it now is on the Forms. But withal it is a more pessimistic view, emphasizing rigorous indoctrination and the impossibility of the many ever achieving genuine knowledge and moral autonomy. Plato's view darkens still further in the later works, the *Statesman* and the *Laws:* here even the philosopher ideal is largely abandoned. In his old age Plato settles for a "second-best" rule of law. Genuine virtue now appears beyond all reach.

How much of the mature Plato (i.e., the Plato of the *Republic)* is already present in the early dialogues is not made clear, but Klosko does concede something to the so-called "unity" view of the corpus: Plato "points in the direction of" his mature philosophy and "hints at the shortcomings" of Socratic thought, even in the early works. But the emphasis is on change, and at the center of this is the psychology. Although Klosko does not explicitly assign causality, the suggestion is strong that the other facets of Plato's though moved in response to changes in this part.

There are, perhaps, some minor quarrels. Why a change in the psychological views should have entailed a change in the epistemology and metaphysics—particularly why the tripartite soul should have necessitated the theory of the Forms and the scheme of education described in the *Republic*—is not entirely clear. Klosko generally urges a connectedness to these various developments but sometimes leaves the precise connections somewhat obscure. More attention, too, might have been accorded to the theological underpinnings of virtue in both Socrates and Plato—Socrates' divine voice and what may take its place in the *Republic*, the persistently mysterious Form of the Good, shored up in some measure by the myth of the afterlife in Book 10. Such a treatment might have entailed a greater emphasis on the continuities between the early and middle strata, at least by showing Socrates to have been less one-sidedly intellectualistic than Klosko find him to be.

But these are points of emphasis, not wholesale criticisms. Klosko has offered political theorists a work that is long overdue—a comprehensive discussion of the political theory embadded in the whole Platonic corpus. Nothing on its scale has appeared since Barker's classic *Greek Political Theory* (1918), and few scholars are better equipped to do it today than Klosko. His reading is sympathetic—some fine comments are reserved in defense of Plato against his critics, notably Popper—and his scholarship is impressive. The work will serve well those whose interest in political theory originates with its awakening in the Greek world of Socrates and Plato.

CURTIS JOHNSON

*Lewis & Clark College*

**Alexis de Tocqueville: His Social and Political Theory.** By Whitney Pope (Beverly Hills: Sage, 1986. 210 p. $16.95, cloth; $8.95, paper).

Pope observes, toward the end of this brief book, that Tocqueville works "within what he considers an established tradition of history, social science and moral philosophy dating back at least to the ancient Greek philosophers" and that he "feels no need to establish a new science" (p. 109). Despite recognition of this fact, Pope's own approach to Tocqueville is largely dominated by the assumption that an authoritative body of social-science theory developed chiefly in the twentieth century provides the best framework for arguing Tocqueville's importance. Tocqueville, he suggests, has "no large body of devotees" in current social science and has been "widely slighted"; the depreciation is likely due to the "widespread perception that he fails to employ, much less clearly state, a theory" (p. 133; cf. pp. 19-22). To remedy the neglect, Pope attempts to find the elements within Tocqueville's work that anticipate more current terminology, concepts, and methods. Thus, in a chapter on "Tocqueville and Contemporary Social Science," we learn about Tocqueville and his "comparative-historical approach" (p. 32), the "method of concomitant variation" by which "Tocqueville tested for cause-and-effect relationships" (p. 34), Tocqueville's use of "two different logics of control" (p. 40), and so forth. The subsequent chapter attempts to compare Tocqueville with Marx and Durkheim and, similarly, tends to read him through an understanding of problems and concepts that is theirs, not his. As one might expect, this road to Tocqueville is rather ineffective in highlighting what is unique in his work. It also minimizes the interesting possibility that Tocqueville's thought could be notable less as an anticipation of contemporary social-science theory than as a critical alternative to it. In addition, one might well question the attempt to read Tocqueville as a theorist in this sense. If Tocqueville thinks that "social science is an analytical science," as Pope claims, he also holds that it must be "useful" (p. 30). Surely the practical concern is finally at least as important to Tocqueville as the theoretical, if not more so. Cited above is Pope's observation, correct in the context, that Tocqueville was not interested in the founding of a "new science," at least in the sense in which Parsons, for example, means that term (see p. 21). But in another sense, Tocqueville called explicitly for a new political science able to guide the democratic revolution and shape its consequences. This kind of science is marked not by an abstract, theoretical character but by its orientation toward the practical improvement of democracy.

Readers needing an introduction to Tocqueville and a sense of the breadth of his thought may gain something from this study and its wealth of citations. Yet the many quotations often demand more in the way of interpretation than is given by Pope, and the manner of presentation in any case gives little sense of the political and stylistic lucidity that distinguishes Tocqueville's writing.

DONALD J. MALETZ

*University of Oklahoma*

**John Stuart Mill's "On Liberty."** By John C. Rees. Edited by G. L. Williams (Oxford: Oxford University Press, 1985. xi, 210 p. $19.50).

John C. Rees planned but never completed a book on Mill, and this volume assembles essays (half of them previously published) drafted as chapters dealing with the argument of *On Liberty*, "what people have thought about those arguments and how they stand today" (pp. vi, 4-5). Rees argues that the central, distinctive doctrines of *On Liberty* are not at odds with Mill's other mature writings nor with his utilitarian inheritance (pp. 5, 10-13).

The most interesting and novel part of the book is the first two chapters (both previously unpublished) where Rees argues that James Mill's essays on government, jurisprudence, and liberty of the press are the primary source of distinctive views in *On Liberty*. First, Rees's careful critical examination of the texts reveals paternal influence on John Stuart's "departure from a strictly utilitarian treatment of social values" (pp. 18-19), "the notion of an intellectual elite exercising an important social and political role" (p. 20), a concern for the coercive power of public opinion (p. 27), and the appeal to hurt or injury to others as necessary for legitimate legal or popular sanctions that limit individual liberty (pp. 22-28). Secondly, Rees shows that Mill's writings during the 1820s already reflect his father's influence with respect to liberty (p. 52), and that concern for individuality is explicit in a letter Mill wrote eight weeks before meeting Harriet Taylor (pp. 70-74). Thus, central doctrines of *On Liberty* are not even primarily the product of Taylor's influence but actually are part of an intellectual inheritance from James Mill.

Chapters 3 and 4 are previously published pieces that review critics' responses to *On Liberty*. Rees surveys "the opinions of Mill's contemporaries on his essay" (p. 73), revealing a critical reception for *On Liberty* (p. 79). And then Rees dispenses with Himmelfarb's thesis of the "two Mills" (pp. 109-15), Feyerabend's charge of inconsistency between *On Liberty* and Mill's own philosophy of science (pp. 115-25), and Cowling's "moral totalitarian" interpretation of Mill (pp. 125-36). Rees's arguments are compelling in each of these cases.

The final two chapters sketch an interpretation of *On Liberty* itself. Chapter 5 is actually an influential paper in which Rees argues that Mill understands other-regarding acts as those "affecting the *interests* of others" (pp. 142, 148). And in Chapter 6 Rees argues that, on Mill's view, the rules of justice "constitute precisely that set of rules which secure the members of society from harm, hurt, or injury" (pp. 160, 166-68), that these rules specify rights (p. 165), and that these rights protect essential, paramount, or vital human interests (pp. 161-62). Thus, Rees concludes that there is an important connection between *Utilitarianism* and central doctrines of *On Liberty* (p. 156).

Rees's fine scholarship contributes to an improved understanding of Mill's utilitarianism. First, Rees's careful critical examination of texts exemplifies an important methodological principle for which he also argues: a philosophic interest is not at odds with, but is essential to, fruitful study of the history of political thought (pp. 1-4). Secondly, the interpretation of Mill's own texts (chaps. 5-6) are aimed in the right direction, and Rees occasionally hints correctly at the importance of exploring Mill's conception of utility (pp. 19, 29-32, 74-77); however, similar views are effectively developed in greater detail by Fred R. Berger in *Happiness, Justice, and Freedom* (Berkeley: University of California Press, 1984) and by John Gray in *Mill on Liberty: A Defence* (London: Routledge and Kegan Paul, 1983).

This volume is disappointing in at least two respects. First, given its nature, much of the scholarship is now dated (e.g., chap. 5), and the essays do not cohere as a book. But more significant is a substantive omission in the argument. Rees effectively argues for a historical thesis about paternal influences on the views of *On Liberty*. But, as Rees recognizes (pp. 10-13), many critics see the defense of liberty as a logical departure from Mill's utilitarian inheritance. Rees never addresses the logical issue: are the doctrines of *On Liberty* consistent with utilitarianism? Even if Rees is correct about the connection between Mill's accounts of liberty and justice (chap. 6), he never considers the old worry about the latter's compatibility with utilitarianism. Thus, despite its merits as historical investigation, in an important sense Rees did not complete his announced task of showing the compatibility of *On Liberty* with Mill's other mature writings and with his utilitarian inheritance.

ROBERT W. HOAG

*Berea College*

**The Definition of Moral Virtue.** By Yves R. Simon. Edited by Vukan Kuic (New York: Fordham University Press, 1986. xiv, 137 p. $20.00, cloth; $10.00, paper).

Although he did not live a long life, Yves Simon (1903-61) was immensely productive. The works published during his lifetime—especially *The Philosophy of Democratic Government*—are well known to political theorists. Thanks chiefly to the efforts and expert editorial skill of Professor Vukan Kuic at the University of South Carolina, Simon's unfinished manuscripts *The Tradition of Natural Law* and *Work, Society, and Culture* were published in 1965 and 1971, respectively. Now Kuic has given us a third volume.

The present book presents Simon's lectures in a course on "Virtues" offered for the University of Chicago's Committee on Social Thought in 1957. The lectures, along with Simon's extended replies to students' questions, were recorded.

One can give no better summary of the work than that by Vukan Kuic himself: Yves Simon "shows how much better contemporary problems can be understood . . . if . . . approached in the light of the great teachings of the past. . . . Simon readily acknowledges the advances made . . . since . . . Plato. But giving Marx or Freud their due—or Hume or Rousseau—does not require taking . . . away from Aristotle. Simon shows how knowing . . . the classics . . . enables us to give that much more credit to modern thinkers—where and when they deserve it."

Simon's own preintellectual virtues emerge clearly in this book. He seeks to relate the concept of moral—as distinct from intellectual—virtue to the reader's personal experience. He follows truth wherever it may lead (even if it means praising Proudhon in one striking instance). He avoids stereotyping and negatively categorizing his opponents. He brings alive for the general reader what could have been a dry and arcane discourse directed only at specialists in Aristotle and Thomas Aquinas.

Simon distinguishes moral virtue from habit, scientific *habitus*, and opinion. His exposition of why *habitus* in Aquinas should not be translated "habit" but should be left as it is (because no English equivalent exists) convinces this reviewer.

By way of criticism, I find Simon's emphasis on "dependability" as the chief mark of moral virtue to be questionable. After all, Adolf Eichmann was quintessentially "dependable." Were he alive, Simon would probably respond with a reminder that, while *analytically* distinguishable, intellectual and moral virtue are *existentially* united in the mind and character of the *spoudaios* (good person).

I recommend that Eric Voegelin's essays "What Is Nature?" and "Reason: The Classical Experience" in his book *Anamnesis* (translated by Gerhart Niemeyer [Notre Dame, IN: University of Notre Dame Press, 1978]) be read in conjunction with Simon's analysis. The interpretations of Aristotle's terms *phronesis* (practical reason) and *physei dikaion* (right by nature) by these great scholars complement one another superbly.

DANTE GERMINO

*University of Virginia*

**Toynbee's Philosophy of World History and Politics.** By Kenneth W. Thompson (Baton Rouge: Louisiana State University Press, 1985. 230 p. $27.50).

This is a thoughtful study of Arnold Toynbee's philosophy of history, his views of international relations, and the relevance of these to international-relations theory. The author skillfully shows the implications of Toynbee's massive *A Study of History* for the development of his views on international relations, but he also underlines the degree to which Toynbee's philosophy of international politics was affected as much by the times in which he lived as by his historical researches. A staunch romantic nationalist before 1914, Toynbee was pushed by the calamitous Great War into the kind of cosmopolitan idealism and advocacy of collective security and international organization that was prevalent in many intellectual circles during the interwar years. Professor Thompson shows, however, that during the 1930s Toynbee began to move beyond such perspectives towards a kind of Christian political realism in which he remained the rest of his life.

He was confirmed in this philosophical evolution by several developments. He came to perceive that the West was undergoing a profound spiritual, cultural, and political crisis, of which Hitler's barbaric militarism and the West's own confusion were but a few dimensions. The depth of this crisis led him to a less optimistic view of human nature and of politics than that implied in the classical liberal schemes for international cooperation and collective security. This, in turn, moved him to reexamine the vast canvas of his own *A Study of History*, where he found "laws" or patterns of international politics whose recurrence the twentieth century could ignore only at its own peril: the ubiquity of the struggle for power among nations, the problem of war, the operation of the balance of power, and the processes of diplomacy as a means for mitigating or altering the course of the struggle for power. Confronted in the late 1940s and 1950s with the Cold War, the atomic bomb and the prospects of a ruinous war between the Soviet Union and the United States, Toynbee thought that the West should look to these "laws" or principles and to the rich course of history itself, rather than to abstract idealizations such as the United Nations, as a source of political wisdom. He urged the West "to gain time" by pursuing a moderate, judicious diplomacy towards the Soviets, lest we destroy ourselves in one of those catacylsmic wars such as had spelled the end of many civilizations in the past.

Professor Thompson is well aware of Toynbee's limitations, and of the selectivity with which he focused on portions of the historical data to support his theories of historical evolution. Toynbee's claims notwithstanding, his philosophy of history was far from scientific or fully empirical. Yet, his work is valuable to historians and political scientists. *A Study of History* was and remains a challenge to the narrowness of much modern research. Toynbee asked the political scientist to see international relations in the context of millennia of historical development, and the political scientist will want to scrutinize and test the validity of Toynbee's numerous concepts. In the process of so doing the student of international relations will be enlarging his understanding and perspective of the field. As Professor Thompson suggests, many of Toynbee's observations "illuminate some of the shadowy corners of history which scientific studies have left untouched."

In a deeper sense, this book presents a challenge of its own. For the most part, international-relations theory has paid close attention to the work of political philosophers (Hobbes, Rousseau, Kant, Marx, Mill), while being less solicitous of the potential contributions of great historians. In this impeccably scholarly work, Professor Thompson demonstrates that historians such as Toynbee have a major part to play in the great debate among international-relations theorists on the questions of whether there are recurrent patterns in the course of international relations and, if so, what the nature of those patterns happens to be.

ALBERTO R. COLL

*Georgetown University*

**The Authoritative and the Authoritarian.** By Joseph Vining (Chicago: University of Chicago Press, 1986. xix, 261 p. $25.00).

The title to this book indicates its aim to distinguish between the *authoritative*, to which respect is intelligently and freely given, and the *authoritarian*, to which obedience is irrationally and blindly given. The author is concerned above all to affirm law as authoritative, as a set of institutional relationships whereby a community constitutes and educates itself. Legal analysis presupposes mind, and legal analysts search through texts to discover that mind. The search cannot be restricted to the words of a text but necessarily extends to the mind of the speaker responsible for the words. The author explores the relationship between legal method and institutional structures, the place of hierarchy in legal thinking, and the importance of a central focal point in creating legal authority. In the latter connection, the author notes disapprovingly the increasing bureaucratization of the U.S. Supreme Court, where clerks write the Justices' opinions and the Justices produce Court opinions by patchwork from what the clerks have written.

In stressing the authority of law, the author is at pains to reject process or behavioral theories insofar as the latter are reductionist. He is not at all antagonistic to empirical-social-science approaches to law, but he argues that the authoritative character of law cannot be reduced to process or behavior or institutions. The distinctive characteristic of law is the respect and willing obedience lawyers, judges, and citizens accord it. The author similarly rejects an approach that restricts itself entirely to exegesis of texts without taking into account speakers and listeners. The author draws an analogy between law and theology insofar as lawyers and theologians alike appeal to authoritative texts and make an act of faith in the minds behind those texts.

This brief summary of the author's views hardly does justice to the complexity of his argument. His method is basically phenomenological and personalist. He is concerned with the way in which lawyers and judges search for meaning in legal documents and the way in which citizens react to pronouncements about law. The argument is both objectively oriented toward the content of law and subjectively oriented toward speakers and listeners in the legal system.

This reviewer agrees wholeheartedly with the objections that the author advances against theories that would reduce law to process or behavior or institutions. In the name of empiricism, reductionist theories dismiss the content of law from consideration. Yet the content of law and the concern of lawyers and judges for it are surely part of the empirically given, arguably the most important part. The author also, in my view, effectively argues against philosophies that would completely relativize the content of law (relativism) or completely subordinate the content of law to subjectively desired ends (pragmatism).

While the author's thesis and main line of argument are convincing, he provides no rationale for the authority of law that he defends. To say that law is authoritative is true enough and a necessary corrective of theories that dismiss or denigrate the authority of law. But why *should* law be authoritative?

Philosophers like Aristotle and Aquinas, for example, ground the authority of law in its necessity for genuine human fulfillment. They identify activities of reason and activities according to right reason as specifically human activities, they make intellectual and moral well-being central to the purposes of human society, and they relate law to justice. In short, for them, law should be authoritative if it is humanly fitting and just. Because of the author's phenomenological method, he rests content with the assertion that the alternative to law as authoritative is madness. Surely more could be said.

The analogy of lawyers' method to that of theologians, however suggestive, may be misplaced. While lawyers, like theologians, appeal to authoritative texts, do lawyers need to make an act of faith in order to regard the texts as authoritative? Cannot reason itself supply a point of departure in human ends and the purposes of human society?

The author's limited thesis also precludes consideration of how lawyers and judges should "find" the content of law. What should be the canons for interpreting law? How much weight should be accorded words of texts, intentions of framers, consequences, social justice? One does not have to agree with Attorney General Meese's thesis in order to accept the importance of the question he raised. The failure of the author to suggest rules of interpretation seems curious in view of his avowed purpose to defend the authority of law.

This book is not easy reading, but it is rewarding. I recommend it to those interested in the philosophy of law and to social scientists working in the area of law. For the latter, it will be valuable for its insights into the method of lawyers and judges as well as heuristic for behavioral research.

RICHARD J. REGAN

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# AMERICAN POLITICS

**The Politics of State and City Administration.** By Glen Abney and Thomas P. Lauth (Albany: State University of New York Press, 1986. xvi, 260 p. $10.95, cloth; $5.50, paper).

Abney and Lauth have undertaken a thoughtful and timely study of interactions between state and city administrators and other local political actors. These include chief executives, legislatures, budget officers, interest groups, and the judiciary. By examining perceptions of the influence of these various political actors on administrative decisions, the authors make an initial contribution toward filling a lacuna in subnational politics.

The volume is divided into two sections that separately examine state and city administrators. The state section is the stronger of the two. The analysis reported in this section is based primarily upon the authors' survey of 778 state-government department heads. Using this survey data, the perceived influence of state executives, legislators, and interest groups on administrators' decisions and behavior is systematically examined.

A number of interesting relationships are uncovered. After examining the perceived influence of governors on administration, the authors conclude that state executives are more "managers" than "policy leaders" and are reluctant to exercise their formal powers to impose their priorities on state administrators. Likewise, administrators are often able to ameliorate the harmful effects of legislative intervention through agency lobbying as well as through mobilizing allies such as local officials and interest groups.

In examining the relationship between state administrators and their political environment, the formal rules of the game are often salient. Organizational characteristics of state government as well as the institutional powers of political actors affect the amount of political intervention into state administration. The first section concludes with a brief analysis of state budgetary policy. The analysis reported in this chapter is based upon the authors' surveys of 48 executive and 40 legislative budget officers.

The second section of the book focuses exclusively on city government administration. This section develops and tests a number of interesting hypotheses regarding the impact of structural features of local government on administration. In particular, chapter 8 goes beyond conventional discussions of reformism and examines the effects of specific organizational characteristics and formal powers of municipal executives on the values and decisions of local-government administrative officials. The distinction between formal and informal executive power developed here is also useful.

Unfortunately, the conclusions drawn in this section are limited because the analysis is based upon a survey of only police chiefs, fire chiefs, and public-works directors and not of all department heads, as was the case in the state survey. Moreover, the three agencies examined provide routine "housekeeping" services. Inclusion of agencies with redistributive or economic development functions would have added greatly to the study. Because economic development and redistribution are more controversial and have identifiable and more easily mobilized constituencies, the relationship between these agencies and the local political environment may be vastly different from that of housekeeping agencies.

Within the limits imposed by their data, the authors examine a number of interesting propositions. For example, the ability of municipal executives to exert influence over these line agencies is found to be a function of both formal executive power and personal style. Perhaps the most significant accomplishment of this section is its contribution to our understanding of the importance of the formal structural arrangements—especially those emanating from the municipal reform movement—on local administration. While it is clear that structural factors have only limited value in predicting expenditures, the analysis provides support for structural effects on administrative values, political control, and functional priorities.

Abney and Lauth have constructed a book that despite its limitations addresses a wide range of issues in state and local administration that have been previously neglected. Although the analysis is not highly sophisticated, its reliance upon contingency techniques and tabular presentation makes it ideally suited as a text for an undergraduate or Masters in Public Administration course on state and local politics.

RICHARD C. FEIOCK

*University of Miami*

**Change and Continuity in the 1984 Elections.** By Paul R. Abramson, John H. Aldrich, and David W. Rohde (Washington: Congressional Quarterly, 1986. xvi, 322 p. $11.95).

Paul Abramson, John Aldrich, and David Rohde have written a book on the 1984 elections that is a pleasure to read. Emphasizing what they call a "social-scientific analysis" (p. xv), the authors investigate several aspects of presidential, House, and Senate elections: historical patterns of voting behavior and turnout, the specifics of public opinion and voting behavior in the 1984 context, and the longer-term meaning of the Reagan era for U.S. politics. Is the United States undergoing a partisan realignment that will make Republicans the majority party in the future? Is it best characterized by dealignment? Or are there more subtle changes taking place in the political process?

The strengths of this volume lie in its organizational clarity and the comprehensiveness of its empirical documentation on voting behavior. The book is organized in four parts (similar to its companion pieces on the 1980 and 1982 elections): a description of candidates, events, and strategies in the 1984 presidential race, an empirical assessment of public opinion and voting behavior at the presidential level, a discussion of House and Senate results in 1984, and a final chapter analyzing several future scenarios for U.S. elections.

Abramson, Aldrich, and Rohde mix a wealth of survey research with interesting commentary on individual candidates and particular events in 1984. Their evidence shows, for example, the limited role that issues played in Reagan's victory and the far more important role that job performance and economic conditions played. They also note that the general election was over before it started because so many voters made up their minds by convention time (pp. 61-62) and that "increase-the-turnout" efforts on the part of Democrats were doomed to failure because of difficulties in mobilizing nonvoters, countermobilization efforts by Republicans, and the low support that Mondale had among nonvoters (pp. 122-23).

These findings help observers understand Reagan's victory, but they also raise a host of issues that will occupy students of U.S. politics for some time to come. For example, beyond incumbency effects in the House of Representatives, why have Republicans done so much better electorally at the presidential than at the congressional level? Why do U.S. citizens love President Reagan but not his policies? What will be Reagan's electoral and policy legacy after he leaves office?

Abramson, Aldrich, and Rohde present results that are relevant for this debate but ultimately leave the last question unresolved because their primary focus is the 1984 elections. Students of U.S. politics will want to pursue the issue of Reagan's legacy in further detail, in both the electoral and the policy arenas. But this point notwithstanding, I would recommend this book to all who follow elections and especially to those interested in a supplemental reading for undergraduate courses on U.S. elections.

DARRELL M. WEST

*Brown University*

**Managing Macroeconomic Policy: The Johnson Presidency.** By James E. Anderson and Jared Hazleton (Austin: University of Texas Press, 1986. xii, 285 p. $27.50).

Several of the political-science rubrics could be called upon to characterize the work under review, and each would be correct to a point. Yet an accurate assessment of the work must finally mark it as the product of a combination of conceptual orientations. First and most obvious, it is a case study concentrating on the Johnson presidency. In addition, it is a policy study that examines a domestic arena long given rather short shrift by political scientists, that of macroeconomic affairs. (This professional inattention to a vital policy area is now being reversed with a rapidly growing literature of research and analysis. See, for example, James P. Pfiffner, ed., *The President and Economic Policy* [Philadelphia: ISHI, 1986].) Finally, *Managing Macroeconomic Policy* is a study of the institutionalized presidency. As such, it finds its bearings in the organizational and managerial perspectives. It is in this latter focus that the unique resource tool, archival research, is utilized with handsome rewards. Thus, the major data here analyzed are those agenda, minutes, reports, letters, memoranda, and oral history interviews to be found in the White House Central Files and related papers at the Lyndon B. Johnson Presidential Library in Austin, Texas.

Reflecting the organizational-institutional viewpoint, the authors begin by elaborating on the modern emergence of an economic subpresidency. Institutionalization in this instance is marked by the routinization of roles and perspectives motivating elite presidential staffers responsible for both formulation and implementation of macroeconomic policy. An inner circle grouping of the chiefs of Treasury and Budget, Economic Advisers, and the Federal Reserve expands with need and circumstance to include less routinely committed actors such as, inter alia, cabinet-level officials from Interior, Labor, the Trade Negotiator's office, and top White House assistants. The regular forecasts, analyses, and policy recommendations of the tetrad (Treasury, Office of Management and Budget, Council of Economic Advisers, and the Federal Reserve) have reflected institutional presence since the Kennedy-Johnson years. By and large, presidential utilization and management of these policy-driving forces is the contemporary definition of policy leadership.

Not only are institutional relationships stable in this policy area, but the definition of appropriate policy strategies is conventional. Thus, the authors proceed to examine systematically the Johnsonian twists in budgeting/taxing (fiscal policy), money and credit (monetary policy), and in wage-price influence mechanisms (incomes policy). They further contend that the management of foreign economic policy (largely trade and balance-of-payments decisions) was of such vital national macroeconomic significance in the Johnson era as to merit extensive analysis along with the other economic policy repertoires.

Needless to say, a concentration on administrative structure, official documents, and paper transactions of the archival record *could* lead to an arid, official chronicle—formalistic and tediously descriptive. Such is not the case in *Managing Macroeconomic Policy.* While there is no overall theoretical or conceptual purpose in the work, it is replete with instructive insights into various aspects of institutional policy formulation such as the focusing quality of common intellectual backgrounds of staff professionals who come at problems from varying institutional role perspectives (p. 104); the differing bases of decision-making influence exploited by different institutional actors (legal prerogatives, professional prestige, proximity to the president, etc.) (pp. 76, 84, 98, 170); as well as "the politics of" in a variety of settings (recruitment, decision making, coalition building, and merchandising) (pp. 75, 106, 107-8, 167).

Finally, since this is a case study, what do the authors find particularly Johnsonian in nuance? Several qualities predominant in the Johnson record stand out. First, the president was actively, personally, attentively involved in each of the important decisional processes. Second, Johnson exhibited a thirst for knowledge in an area of minimal training or experience. Typically, he sought multiple advice sources, differing interpretations of the data, and contrasting recommendations. Third, from all of this he demanded consensus as an end product and would tolerate no holdouts among his advisers. Finally, the president's experience-born political sensitivity always took precedence. Months of policy analysis and professional certainty could be swept aside when Johnson made a personal determination of political infeasibility. Thus the authors (p. 228) accept Council of Economic Advisers Chairman Ackley's explanation (p. 60) for the president's baleful refusal to support a Vietnam-necessitated tax hike. Johnson was convinced that the technically justified proposal could not win in Congress.

CHARLES E. JACOB

*Rutgers University, New Brunswick*

**The Resurgence of the States.** By Ann O'M. Bowman and Richard C. Kearney (Englewood Cliffs, NJ: Prentice-Hall, 1986. x, 269 p. $19.95).

Looking at the U.S. states in 1933, Luther Gulick, renowned "dean" of U.S. public administration, asked if the states were any longer "the appropriate instrumentalities for the discharge of . . . [important domestic] functions." The answer, he thought, was "not a matter of conjecture or delicate appraisal. It [was] a matter of brutal record"—they were not ("Reorganization of the States," *Civil Engineering,* August 1933, 420). Others echoed Gulick through the years, none more damningly than Terry Sanford, who declared in his *Storm Over the States* (New York: McGraw-Hill, 1967), that the states then were "indecisive," "antiquated," "timid and ineffective," "not willing to face their problems," "not responsive," and "not interested in cities" (p. 1). In short, expert opinion had come to despair over the states.

Twenty years later, the authors here assert, it has come to pass that the states, far from falling into desuetude, have become central to the public-policy process on the domestic front in the United States. Indeed, that is the case that Bowman and Kearney set out to prove in their book. As they see it, "the states have experienced a revitalization" and must be regarded now as "key elements in the American democratic system" (p. ix).

To prove their assertion of resurgence, they first sweep across U.S. federalism in transition to a seriatim presentation of the "precipitating factors" of state revitalization therein and of the beneficial products of each of those factors. These precipitating factors include such things as "lack of confidence in the federal government," "constitutional reform," "intergovernmental lobbying," "reapportionment," and "local government incapacities." Moreover, the authors recount various beneficial products of revitalization, including improved revenue systems, faster diffusion of innovation, increased responsibility for local governments, and the expanded scope of governmental operations.

To drive their point home, the authors then turn in successive chapters to resulting reforms in the executive, legislative, and judicial branches of state government, enhanced citizen participation in state politics, and the new climate of cooperation in state-local relations, replacing the old climate of conflict—though they admit there are still a number of critical issues in state-local relations. They attempt to show state innovation at work in key policy areas: economic development, education, and hazardous and nuclear waste. In a concluding chapter, the authors summarize their argument and make predictions for the future. Each chapter is followed by an extensive list of references, and there are both state and subject indexes.

The argument the authors make is not original. It is advanced in extensive corroborating detail in a 1985 study by the Advisory Commission on Intergovernmental Relations, *The Question of State Government Capability* (ACIR Report no. A-98 [Washington: GPO, January, 1985]) and spottily in the daily and weekly press and in a number of other studies. But for a well-written, carefully constructed, thoroughly documented, and utterly convincing overview of what may be the major governmental phenomenon in the United States in the last quarter of this century, Bowman and Kearney's book will do very well. It has obvious utility as a text book, but it merits a larger lay audience as well. For the impetus of Reagan's New Federalism has not yet spent itself, and it may well be the states and their local governments that will have to carry the major burden of domestic governmental action for a good many years ahead. The authors give us assurance that they will be able to do so.

RICHARD H. LEACH

*Duke University*

**The Search for Government Efficiency: From Hubris to Helplessness.** By George W. Downs and Patrick D. Larkey (Philadelphia: Temple University Press, 1986. vi, 273 p. $27.95).

In recent years, political scientists have become more open-minded about government bureaucracy. Whereas in the past they almost always attacked it for being uncontrolled, suppressive of the individual, or not sufficiently marketlike, today several "revisionist" treatments are appearing that picture the bureaucrats as not so evil after all. This book falls into the latter category.

Downs and Larkey do two things in this book: (1) they discuss on a conceptual plane broad issues related to efficiency and effectiveness in government; and (2) they make a number of technical critiques of policy or management analytic methods, such as benefit-cost analysis, program budgeting, management by objectives, and performance appraisal. While both of these realms of discussion are meritorious in many ways, the reader gets the uneasy feeling that the book is comprised of two distinct modes of analysis, a problem that may stem from coauthorship.

This is not to say that the two modes of discussion are not each well developed, however. The conceptual theme of the book is that U.S. government is more efficient than is commonly believed, while its popular standard of comparison—U.S. business—is less efficient than many assume. With respect to the analytic methods, the authors point out how each is limited by value disagreements, measurement problems, and the inevitable play of politics. These contentions are hardly new but are worth repeating.

The authors conclude that while government performance can definitely be improved, we should not expect perfection and must be suspicious of all panaceas. Yet the writers themselves advance the ideal of detached analysis in government by proposing a super watchdog agency, modeled after the General Accounting Office (GAO), that would be funded outside the appropriations process and run by a board appointed by Congress with the consent of the president. The constitutionality of this suggestion would seem even more suspect than Gramm-Rudman.

The most original part of the book is its scathing critique of the Grace Commission and its recommendations. In a statistical appendix, it is shown that many of the corporations that contributed executives and money to the Grace enterprise are themselves relatively poor performers in their respective industries.

Two aspects of the book are puzzling. First, why "From Hubris to Helplessness"? U.S. citizens were never overly proud of their government's efficiency, and the continued popularity of managerial cure-alls does not suggest a sense of resigned helplessness. Second, why did the authors ignore their fellow revisionists? No mention is made, for example, of books by Christopher Hood, Daniel Katz, Herbert Kaufman, Victor Thompson, and John Weaver, and this reviewer. The same is true for articles by John Fisher, Alvin Gouldner, Stephen Miller, Brint Milward, Zahid Shariff, and Michael Wriston. As revisionists, Downs and Larkey are in wider company than they know or care to acknowledge.

CHARLES T. GOODSELL

*Virginia Polytechnic Institute and State University*

**How Real Is the Federal Deficit?** By Robert Eisner (New York: Free Press, 1986. xv, 240 p. $17.95).

By showing us how to measure the federal deficit better and by a rich demonstration that the government has assets as well as liabilities, Robert Eisner has written an essential book for all students of public finance. By neglecting the political significance of the debt, however, Eisner does not prepare his readers for the likely effects of attempting to implement his ideas.

Eisner propounds a dual thesis: (1) The deficit has been mismeasured largely because of failure to take into account changes in the level of prices and capital assets; and (2) this measurement rescues Keynesian economics from charges that it is responsible for stagflation. Deficits do matter in the way Keynesians think, in short, but only if they are measured correctly.

The wisest judgment on the economic impact of deficits, in my opinion, is the Scottish verdict: unproven. All efforts to relate the relative size of deficits to bad things—unemployment, inflation, high interest rates, trade imbalances, you name it—have failed. The likely reason for this indeterminancy is that some important variable—the international supply of money, the total resources of U.S. citizens, the cost of capital—has been left out or misspecified. Nothing new there. Whatever the economic significance of deficits may be, however, their political significance continues to grow.

The deficit has now become the single most potent and the only effective force holding down spending in the past half century. Whether the budget should be balanced at high or low levels of spending and revenue and who should pay how much for what sort of distribution between domestic and defense programs have now become the overriding issues of U.S. national politics. Congress spends as much or more time on these issues than on all other matters combined. Treating deficits as if they were only technical inputs into an economic formula leaves a lot to be desired.

The law of political compound interest states that as payments on the debt grow faster than the budget, new programs are driven out. Taken together with the increase in universal entitlements, Democrats have to be concerned that debt interest will deprive them of all future options except tax increases. Since interest payments go to richer people while welfare payments go to poorer people, moreover, ancient quarrels over the political economy of the debt (shades of Hamilton versus Jefferson) are being revived.

Institutions for implementing policy matter; it is one thing to keep capital accounts and quite another to make them a formal part of the budget. Just as interest payments may drive out substantive spending, so segregating capital from recurrent expenditure is likely to increase both. Experience with European "double budgets" and poor-country "investment budgets" shows that institutionalizing these devices brings new layers of spenders who want to build up good—that is, capital or investment—spending. No one who is familiar with the fiddling surrounding the allowed deficit under the Gramm-Rudman-Hollings Act or the experience of New York City in using bond issues to finance current spending would believe that it is possible to separate categories that policy-motivated officials wish to mingle.

Deficits raise the question of intergenerational equity. Since each generation has been better off than its predecessors, however, it is not clear that it would be right to deprive a poorer present in favor of a richer future. The morality of deficit spending also cuts another way: neither the public nor the politicians would be willing to support current spending, I think, if taxes had to be raised about one-fifth to balance the budget.

Despite its unwillingness either to entertain drastic spending cuts or to raise taxes, the general public remains firmly wedded to budget balance. Balance may be taken as a sign that things are all right, that public officials are doing their jobs, and that agreement among political elites is sufficient for governing. Since balance is an important symbol of security and mechanism of accountability, fiddling with the figures could undermine citizen confidence in government. The Keynesian "full-employment surplus" was one thing; deficits were counted in nominal terms but their meaning in hard times was altered. Now Robert Eisner, White House Chief of Staff Don Regan, and others wish to engage in the most complex kinds of calculations to determine what the "real" deficit actually is. Few will understand (try Eisner) and even fewer will agree. Yet the stability essential for economic growth and political legitimacy may be shaken if the deficit joins the budget base as a contested concept. Before the norm of balance is crushed under this overly warm economic embrace, its political value should be carefully considered.

AARON WILDAVSKY

*University of California, Berkeley*

**Political Parties in the American Mold.** By Leon Epstein (Madison: University of Wisconsin Press, 1986. xiv, 440 p. $27.50).

An innocent reader might think that Leon Epstein has written a textbook. In reality, it is an extended, insightful essay on political parties in the United States that will also serve well as the basic book in advanced and graduate courses.

Epstein's stance is that of an informed and skeptical conservative who emphasizes the persistent impact on U.S. parties of basic public attitudes and of constitutional and institutional constraints. Rejecting extravagant predictions, he states a moderate theme: "I have doubts about both the extensiveness of party decline and the earlier glories of American parties. . . . While not without certain modest hopes for party revival, I am impressed more than are most revivalists by the limits that American circumstances impose on the growth of party capacities" (p. 3).

Beginning with his previous survey of the literature, "The Scholarly Commitment to Parties," Epstein considers eight aspects of U.S. parties: Congressional parties, the presidency, state and local structure, regulation, national organization, identification in the electorate, private funding, and public funding. Each of these chapters presents a coherent argument on the particular topic, while covering basic factual material. In keeping with the character of the book as an essay, there is only a single table (the distribution of party identification) in the body of the volume, while additional data, primarily on campaign finance, are included in eight tables in the appendix. The extensive notes provide a comprehensive guide to the literature of the field.

Epstein emphasizes those factors that have constrained U.S. parties. These include public hostility spurred by corrupt practices, the constitutional separation of powers that has promoted both a "nonpartisan presidency" and electoral autonomy of congressmen, state regulation of voting and party structure and, perhaps most significantly, the direct primary. In discussing the last factor, the author exemplifies his focus on historical and institutional influences. Combined with the decline of patronage and the U.S. insistence on popular participation in all political decisions, the direct primary has created a party system of "institutionalized porousness," where parties are inevitably undisciplined and largely candidate-centered. With his long-term view of these characteristics, Epstein provides a needed lesson to writers who believe that the parties have been radically transformed by television or the post-1968 changes in presidential nominating procedures.

The tone of this book is one of an experienced observer warning us, "Wait a minute, let's not jump to unjustified conclusions." While such caution does not make for apocalyptic prose, Epstein does turn many a neat phrase. Three examples may convey this flavor. Analyzing state regulation of parties, he uses the analogy of public-utility regulation. In the U.S., the political party has been considered "an agency performing a service in which the public has a special interest sufficient to justify governmental regulatory control, along with the extension of legal privileges but not governmental ownership or management of all of the agency's activities" (p. 157). Discussing the alleged decline of party-in-the-electorate, Epstein argues that party identification is not gone but, rather, *frayed,* "a wearing that need not mean disintegration or abandonment. . . . What is frayed may be mended or restored" (p. 262). Reviewing criticism of the Democrats' alterations in nominating rules, he wryly notes that this argument is "necessarily limited because it rests almost entirely on the single case of Jimmy Carter" (p. 103).

Overall, Epstein sees only limited prospects of transformation in the party system, given the strength of the U.S. mold. Where there are such prospects, his attitude is ambivalent. He does advocate some changes, such as more equitable scheduling of state presidential primaries. He questions the utility of other proposed reforms, such as limitations on political-action committees. He worries—too much, I believe—about possible abolition of direct primaries.

Despite Epstein's own informed skepticism, his book does provide some reason for optimism on the part of those who favor stronger U.S. political parties. He provides evidence of more active congressional parties, more professional organizations, new sources of funding, the replacement of parochial local machines by national but federalized parties, and adaptation to new methods of campaigning. The historical and institutional mold is pliable, not iron. Yet, the ultimate virtue of this book is that it fosters such debate even as it instructs all political scientists, whether teachers, practitioners, or students.

GERALD M. POMPER

*Rutgers University*

**Changing Patterns in American Federal-State Relations during the 1950s, the 1960s, and the 1970s.** Edited by Lawrence E. Gelfand and Robert J. Neymeyer (Iowa City: University of Iowa, 1985. x, 82 p. $9.95, paper).

This very slim volume is the product of a conference convened in October 1984 at the Center for the Study of the Recent History of the United States, a consortium involving the Iowa State Historical Department, the Herbert Hoover Presidential Library, and the University of Iowa.

The book consists of three papers presented by prominent political scientists specializing in the study of U.S. intergovernmental relations: Deil S. Wright, Michael D. Reagan, and Martha Derthick; and an edited transcript of a panel discussion among three former state chief executives: Warren E. Hearnes, Democratic governor of Missouri (1965-73); Robert D. Ray, Republican governor of Iowa (1969-83); and Milton J. Shapp, Democratic governor of Pennsylvania (1971-79). This latter session was moderated by the University of Iowa's Gregory Caldeira.

Despite the fact that all substantive contributions to the conference proceedings were produced by either political scientists or former participants in the intergovernmental political process, the format and emphases of the monograph highlight historical themes—an unremarkable observation, given the nature of the conference and its sponsor.

The book's title is partially accurate and partially misleading. The subject is indeed "changing patterns"—specifically, the alterations in the intergovernmental landscape during the three decades prior to the current one. However, the materials are not limited to federal-state relations. While the governors' discussion tends to highlight national-state conflict and cooperation, the academic contributors examine developments in intergovernmental relations more generally. Indeed, Wright's major point is that an exclusive federal-state focus is unsatisfactory if one is interested in discerning the major activities and issues of the post-New Deal era.

The essays by Wright, Reagan, and Derthick, which collectively comprise three-fourths of the text, are organized by decade. All three authors adopt a primarily descriptive approach. There is no testing of empirical theories, no systematic examination of the research literature, no discussion of the future (or even the present) of either the theory or the practice of intergovernmental relations. These characteristics of the contributions undoubtedly stem from constraints imposed by the theme, sponsors, and audience of the conference.

Wright's essay, the longest and most detailed of the trio, covers essentially familiar ground. It contains a discussion of how intergovernmental activity was modified during the period between the Roosevelt years and 1960, plus Wright's well-known explanation of the distinctive meaning and characteristics of intergovernmental relations vis-a-vis federalism. The essay, while admirably documented, provides little that is new for serious students of the subject. A similar comment may be made about Reagan's paper, which summarizes the familiar events of the sixties and their significance in much the same fashion as has been done heretofore. Derthick's contribution, while abbreviated and sans citations even for excerpted quotations, is perhaps the most interesting. She summarizes contrary intergovernmental trends of the 1970s, shows succinctly (through reference to the obsolescence of some of her own well-known published work) how some of these developments render earlier generalizations about the system oversimplified and inaccurate, and raises intriguing questions about the suitability of the federal government's role as it had developed by the dawn of the 1980s.

The discussion among the governors is only loosely linked to the issues raised by the academics. Largely anecdotal, this section displays a predictable and partial view of actors in the intergovernmental system (i.e., state-level and executive-centered) and would likely have been improved had the participants been a more heterogeneous sample of practitioners.

This volume is satisfactory as a summary of significant historical developments. It is likely to be less useful to scholars already well versed in the field.

LAURENCE J. O'TOOLE, JR.

*Auburn University*

**Policy Guidelines for Bail: An Experiment in Court Reform.** By John S. Goldkamp and Michael R. Gottfredson (Philadelphia: Temple University Press, 1985. xv, 249 p. $34.95).

This useful case study is designed to discern how bail decisions for defendants charged with serious crimes traditionally have been made in Philadelphia, develop an alternative model for making bail decisions, and test and evaluate the utility of the alternative model compared with traditional practices. The discussion of methodology and field research is relevant to a general audience of social scientists. It will be helpful especially to those who are contemplating related field research but do not have extensive prior experience. The authors develop each of their three purposes in a straightforward, chronological style that gives the reader the sense of participating in the research project as it unfolds. Such an ambitious project required the close cooperation of criminal-justice professionals, especially judges. What is striking is how successful the authors were in gaining this cooperation.

The first stage of the study endeavored to uncover what factors 20 judges used to determine the kinds and amount of bail imposed for selected criminal categories, and the degree of variability among those decisions. Their observations are consistent with results of previous bail studies. Also, the authors claim significant variability among bail decisions made. That is, defendants with similar backgrounds who were charged with similar crimes frequently experienced different bail obligations. This variability was perceived to be evidence of unfairness in the bail decision-making process. The second stage of the project was to develop a model based on a set of shared guidelines that might be used to reduce variability among bail decisions and produce greater rationality and equity in the bail decision-making process. The guidelines were developed through discussions with judges and by applying actuarial techniques to bail decisions made in the first stage. The third stage of the project, testing and evaluating the two models of decision making, demonstrated how significantly the Philadelphia judges cooperated in the research. Sixteen judges participated in the test. Randomly selected, half were encouraged to use the bail decision-making methods they had always used, and half were asked to employ the guidelines model that was developed.

The results of the experiment suggest rather marginal differences between bail decisions based on the guidelines model and those based on traditional methods. What differences do exist may not be due to the guidelines model despite the authors' arguments to the contrary. The reduced variability across decisions among the experimental judges may be due more to the additional time and effort they spent in a group context discussing bail decision making than to the guidelines model.

Another problem is the use of random sampling to select the experimental group and the control group from such a small number of judges. Switching just two judges might have significantly altered the results. Because we know how the judges performed individually in stage one of the project, it might have been wiser to select two sample groups that consciously balanced the degree of variability among bail decisions. But readers cannot evaluate the samples, because we are not informed which judge was in which group, nor do we discover how the decisions of the experimental judges compared with their decisions observed during stage one. These data are so obviously relevant to evaluating the guidelines model that one must wonder why they were not included.

There are additional flaws in the book worth noting. In the first two hundred pages, the bail options are given as "Released on Recognizance" (ROR) or cash. But in the epilogue we are told about a third type of "bail decision that compromised between requiring a cash deposit from the court and . . . ROR" (p. 212). This raises a number of questions about the frequency of such decisions and how they were treated in the experiment. Also, there is no mention of what role, if any, other criminal-justice personnel played in the bail decision making process. The data in some of the tables, figures and appendixes are inadequately labelled. Finally, footnotes urge readers to consult appendixes in other works that should rather be reproduced in some form here to enable the reader more fully to evaluate what is reported. In spite of these criticisms, the book will prove worthwhile to those contemplating field research, in general, and criminal-justice research, in particular.

FREDRIC A. WALDSTEIN

*Bentley College*

**Judicial Conflict and Consensus: Behavioral Studies of American Appellate Courts.** Edited by Sheldon Goldman and Charles M. Lamb (Lexington: University Press of Kentucky, 1986. 320 p. $30.00).

The editors collect 12 articles that seek to explore the nature and intensity, and especially the causes of, and phenomena related to conflict and consensus among appellate court judges. More specifically, the first set of articles addresses the U.S. Supreme Court. Several focus on U.S. courts of appeals. And a final group looks at judicial behavior on state supreme courts. All of the selected articles share the common theme "that diversity of attitudes is related to the dynamics of conflict and consensus on American collegial courts" (p. 277).

In the prologue, the editors make a good case for examining the subject of conflict and consensus (although it is clear that there is more fascination with the former than with the latter). These phenomena shed light on the key role of judges as policymakers. They tell us something about the internal dynamics of collegial courts, and they provide us with key insights into judicial attitudes, roles, and leadership patterns.

One of the real strengths of this volume is its comparative approach. Speaking for many in 1983, Lawrence Baum bemoaned the fact that "scholars in the field have shown little taste for cross-institutional research even within the judiciary. It is notable, for instance, how little research compares federal and state courts" ("Judicial Politics: Still a Distinctive Field," in *Political Science: The State of the Discipline,* ed. Ada W. Finifter [Washington: American Political Science Association, 1983], 205). Among the more interesting comparative findings are these: (1) consensus and conflict in voting patterns vary on the different levels of U.S. appellate courts over time (Several studies related this to the difficulty and complexity of the issues before the courts at any one time); (2) there is some direct and indirect comparative data suggesting that dissent increases with the overturning of precedents; (3) the findings are mixed as to whether there is a relationship between the size of the workload and the rate of dissent; and (4) several studies discovered strong support for the proposition that urbanization is associated with increases in judicial conflict.

Besides the stress on a comparative approach, it is clear that the editors hoped to advance our knowledge of theory as well. It seems fair to give them high marks in this regard. The attitudinal approach, often tested using bloc analysis and cumulative scaling, is buttressed by several excellent studies. For example, David Danelski's study indicates how attitudinal and other approaches can be incorporated into a multicausal model to produce an effective strategy of research. After performing a content analysis of a number of civil-liberties cases, Danelski then used scaling in an innovative manner to examine activist-restraint role conceptions as they related to judicial conflict and consensus.

Another area of theoretical advance was in the relationship between jurists' attributes or background characteristics and their decision-making patterns. However, the findings here were mixed, and it is clear that this is an area where more research is needed. Perhaps more fruitful and consistent were the findings concerning the relation between judicial role and subsequent behavior. For instance, there is an elegant study by Harold J. Spaeth and Michael F. Altfeld which combines opinion analysis and voting patterns to show that while a justice might appear from the language of his decisions to be a believer in restraint, the reasoning used may in fact rationalize the casting of activist votes.

A final area of significant theory building was in the realm of small-group analysis. In looking at the concept of leadership, for instance, a study revealed that when a new chief justice is a relatively weak leader, his colleagues may then actively compete for leadership. Our theoretical understanding of leadership on state supreme courts is enhanced in a study by Victor E. Flango, Craig R. Ducat, and R. Neal McKnight, who analyzed opinion assignments and coalitions in decisions with nonunanimous opinions.

In sum, this reader is not just another collection of articles in book form. By selecting a first-rate team of contributors and by preparing an effective and an insightful prologue and epilogue, the editors have used this text as a significant vehicle for advancing our substantive and theoretical knowledge of judicial behavior.

ROBERT A. CARP

*University of Houston, University Park*

**Shadow Justice: The Ideology and Institutionalization of Alternatives to Court.** By Christine B. Harrington (Westport, CT: Greenwood, 1985. 216 p.).

This book is part of a literature critical of the rapidly expanding alternatives-to-court movement. The author argues that this movement provides, under the guise of informalism, a means of expanding the capacity of the state to maintain public order by intervening in disputes that might otherwise elude the judicial net. She characterizes the contemporary alternatives-to-court movement as a conservative political reaction to the rights revolution of the 1960s, aided and abetted by liberal reformers who, despite that revolution, remain skeptical of the capacity of courts to provide increased access to justice, much less justice itself.

In parts 1 and 2, Harrington asserts that a gap has long existed between the ideology and the practice of informalism. Early in the twentieth century, progressive reformers advocated the establishment of courts that would provide "social justice" rather than mere "legal justice." The fruits of their efforts—juvenile, domestic relations, and small-claims courts—frequently failed to deliver on the promise of better justice, but they did substantially increase the state's role in the management of human affairs. In much the same way, Harrington contends, the contemporary alternatives movement embodies a judicial-management strategy whose primary goal is to create a unified, court-centered system that "increases the capacity of the judiciary to create programs—to become proactive" (p. 66).

The empirical support for Harrington's thesis is found in part 3, a case study of the Kansas City Neighborhood Justice Center (NJC), one of three such programs funded by the Department of Justice in the late 1970s. The center was intended to provide mediation services on a voluntary basis for minor civil and criminal disputes involving persons with continuing relationships. Harrington finds that because the bulk of the NJC's business was generated by referrals from the criminal justice system—prosecutors, police, and judges—most mediation participants were in fact subjected to coercive pressures to accept its services. Other forms of coercion were built into the mediation process itself, including mandatory arbitration if mediation failed to achieve an agreement. Harrington refers to such pressures as "institutionalized voluntarism." Despite (or perhaps because of) these pressures, disputing parties were no more willing to participate in mediation than in adjudication. Thus, the author concludes that the NJC is less an alternative to than "a shadow of" conventional adjudication practices" (p. 171).

The NJC is dead—the victim of federal budget cuts—but the alternatives-to-court movement survives and thrives with state and private funding. As exemplary as Harrington's study of the Kansas City NJC is in its own right, it would be unwise to apply her negative conclusions about one program or model to the movement as a whole. Mediation programs that specialize in civil, rather than criminal, matters are less subject to the charge that they are subtle means of expanding judicial authority. On the contrary, consensual-resolution processes may allow disputants to maintain ownership of their own disputes rather than turning them over to a judge for decision making.

Moreover, while Harrington is undoubtedly correct in cautioning against abandonment of reform efforts that define legal rights as "an important political resource that structures bargaining in informal settings" (p. 173), it is equally important to recognize that such informal bargaining has been and will remain an integral part of the judicial process. The emergence of the alternatives-in-court movement provides an opportunity to understand better the symbiotic relationship between adjudication on the one hand and mediation/negotiation on the other. Neither more justice nor better justice will be achieved exclusively through a rights-based strategy or through reliance on informal dispute resolution. The real challenge to legal reformers is to capture the mixture of formalism and informalism that preserves and enhances the best features of both.

RICHARD J. MAIMAN

*University of Southern Maine*

**Origins of American Political Parties 1789-1803.** By John F. Hoadley (Lexington: University Press of Kentucky, 1986. xii, 257 p. $24.00).

One's first thought on seeing the title of this book is likely to be, "Hasn't this been done before?" The answer is yes—and no. Students of party development have placed the birth of political parties anywhere between 1792 and 1830. John Hoadley argues that this confusion results from the use of varying definitions of party and from failure to distinguish the notion of party from that of party system. Those who would place the orgins of parties in the nineteenth century are looking for institutions that would meet twentieth-century standards. Those who would place the birth of parties as early as 1792 are willing to accept factions as parties, which is equally problematic. Hoadley asserts, correctly, that "any conclusion regarding the emergence of parties is dependent on how the term *party* is understood" (p. 19). He prefers to view party as a continuum based on a developmental process that ranges from factionalism, to polarization, to expansion, and finally to institutionalization. Hoadley's book offers a search for evidence as to when key points on this continuum have been reached.

Attention is given to the development of electoral institutions in the new nation (chapter 3) and this analysis involves traditional historical description rather than quantitative methods. Hoadley concludes that the vagueness and uncertainty of early electoral arrangements inhibited the development of formal party contests for office but that this same vagueness permitted those in the majority in various areas to manipulate local electoral arrangements to their own (partisan) advantage. More central to the argument is the analysis of the development in the House and Senate of voting blocs that exhibited both intrabloc unity and interbloc disagreement (chapters 4-8). The meat of the book is an intensive quantitative and qualitative examination of voting patterns in the first through the seventh Congress. Using multidimensional scaling, Hoadley shows that voting patterns in the first Congress were based more on region than on party, but by the second Congress the polarization of voting based on party was beginning, despite a fair amount of within-bloc disagreement. This intraparty disunity diminished considerably by the third Congress and nearly disappeared by the seventh Congress (Figures 1-14, pp. 62-75).

We are offered as well an analysis of "key votes" in various policy areas, designed to show that an underlying set of attitudes, rather than coincidence, produced these voting configurations. Significant progress along the continuum of party development is demonstrated by votes on the Jay Treaty in 1796, debate over which "brought in themes of presidential authority, economics, frontier protection, and even slavery; as such, it represented a culmination of developments in all issue domains" (p. 140).

This book is addressed to a cross-disciplinary audience. Historians especially will appreciate the meticulous analysis of specific votes in various issue domains and of the behavior of specific individual members of Congress. Political scientists will laud the application of quantitative techniques to a subject most often treated in qualitative fashion, although those accustomed to percentage tables may find some of the tables of raw *N*s disconcerting. A useful addition to the quantitative analysis might have been the calculation of party-support scores for individual members in each Congress coupled with an examination of mean levels of party support for each Congress in the 14-year period. Given the careful attempt to classify members' party affiliations correctly (appendix A), this would have been possible, and for some readers it would have had greater intuitive appeal than the spatial configurations of voting. The book deserves examination by serious students of party development and congressional voting and would be appropriate to graduate seminars on either of these topics. It is far too technical, however, for all but the most sophisticated undergraduates.

PATRICIA A. HURLEY

*Texas A & M University*

**Class Awareness in the United States.** By Mary R. Jackman and Robert W. Jackman (Berkeley: University of California Press, 1983. xi, 231 p. $30.00, cloth; $10.95, paper).

**Divided We Stand: Working Class Stratification in America.** By William Form (Champaign: University of Illinois Press, 1985. xviii, 306 p. $28.95).

Both of these books focus on class and politics and spend more time establishing and then defending their conception of social class than with teasing out the relationship between class and politics. Both, also, are authored by established scholars who share a Weberian view of class as a series of graded status groups and who are empiricists in the best sense of that term. This, however, is where the similarity ends.

Mary and Robert Jackman want to determine if social class is meaningful in the United States. Patterning their book after Richard Centers's *The Psychology of Social Classes* (Princeton: Princeton University Press, 1949), they contend that if classes really do exist as social groups, "then they must exist in the public consciousness" (p. 8). This leads them to study subjective social-class identification.

They use a five-class model: the poor, the working, middle, upper-middle and upper classes. To test this they rely on data from a 1975 University of Michigan national survey ($N = 1914$) specifically designed to test these and similar concerns. They initially find that all but 3.5% of their respondents identify with one class, that 80% feel at least somewhat strongly about belonging to this class, that there is wide agreement across classes as to the class location of various occupations, and that economic and cultural criteria are rated equally in determining class membership. Further, in a comparison of class with racial identification, they conclude that, except for middle-class blacks who identify more as blacks than as middle class, class is more salient than race for most U.S. citizens.

To elaborate their findings, they build a basic model of class identification, consisting of three independent variables: family income, household head's socioeconomic index, and respondent's education; and replicate it in a series of regression equations. This model works well for whites but not for blacks, as the major distinction the latter seem to make is between the poor and the nonpoor.

This model is then used to address various issues. Among their more salient results are the following: (1) people appear to translate their social standing into a class identification in a realistic manner, as those who say a particular criterion is important in class membership weight it more heavily in their own class identification; (2) affective bonds toward one's class and having friends of a similar class significantly affect the relationship between objective and subjective class; and (3) class identification affects social predispositions more than political ones. In fact, for class to have a sizeable influence on politics, one needs to have a strong attachment to that class. The Jackmans conclude that the latter is not surprising given the low level of public discourse on, and organized political expression of, class issues in the U.S.

I only have a few minor quibbles with the Jackmans. First, although they conclude that class outweighs race, this is not the same thing as saying that whites in a particular class see themselves as sharing a similar position with blacks in that class. The Jackmans could have drawn on Milton Gordon's *(Assimilation in American Life,* [Oxford: Oxford University Press, 1964]) concept of "ethclass," a social space consisting of the intersection of class and ethnicity, to clarify this. Second, it might have strengthened their results to replicate their analyses using only those respondents who feel at least somewhat strongly attached to a class.

Overall, the Jackman's book is very good. It is well written, well organized, and well argued, and is a model of careful, thoughtful scholarship. One should not interpret the gap between its publication date (1983) and its review here as any indicator as to the quality. This is must reading for those concerned with class awareness in the United States.

Form's book is broader in scope, as his thesis is that industrialization, instead of homogenizing the working class, is actually dividing it. This certainly is a contention of sizable theoretical and practical significance, and one that will continue to be debated.

Form defines a class as consisting of groups with common generational-mobility patterns. Using this, he defines the working class as consisting of five strata of manual workers: the self-employed, skilled workers, nonskilled workers who work in core industries, nonskilled workers who work in periphery industries, and the marginally employed (occasionally a sixth strata, foremen (supervisors), appears). He documents the existence and importance of these strata essentially by looking at income differences among them (controlling for a host of variables typically invoked to explain income differences). Although this short summary cannot do justice to all his results, using data from the county file, 1/1000 1970 Public Use Sample (PUS), he finds significant income differences among the five strata. It is the existence of these, exacerbated by unionization and racial and gender inequalities, that provides the basis for political disunity in the working class.

Unfortunately, the PUS does not include data on political topics. Therefore, Form was forced to rely on his own survey in Illinois and on the American National Election Study (ANES) and General Social Surveys. Using the former, he discovers notable class differences among professional/administrative, clerical, and manual workers, along with somewhat smaller differences across his five strata within the manual working class on different aspects of economic ideologies (e.g., income equality, fairness to earnings). Overall, however, these five strata are rather weakly related to economic attitudes. Using national data, he analyzes class and voting and party identification, this time using a six-strata class model. My interpretation of this analysis is that supervisors behave more like nonmanual workers and the self-employed and skilled resemble each other, as do the unskilled and marginally employed. Finally, so as to test the generalizability of his results he compares them to a similar analysis he did in France. There he finds that although the French working class is reputed to be more class conscious, there is greater earnings inequality within the French working class and (drawing exclusively on the work of others) that there are low levels of political interest and unity. Form concludes with some speculations on the future of working-class politics.

Despite the large amount of analysis and the broad scope of the work, I found several shortcomings in Form's book. First, it is not a tight package, evidenced by his alternating between a five- and six-stratal definition of the working class. Second, although he is generally quite careful, he is not always so. In this chapter on skilled workers, he takes on the neo-Marxist notion that there has been a "de-skilling" of labor in the twentieth century. Although Form presents some useful historical material on skilled workers, his overall treatment is unsatisfactory for a number of reasons: (1) He focuses only on manual workers, while the "de-skilling" argument applies to the whole labor force. (2) In his central table in his argument (p. 93), he compares the percentage of skilled workers in 1900 and in 1980 within sex categories, finding that the skilled labor increased for each. However, after adjusting for the different sex ratios of the labor force in those two years, one finds that the overall percentage of skilled workers actually went down slightly. And (3) his comparison of

France and the United States is curious, since these two countries are sufficiently dissimilar to permit an uncomfortable number of alternative explanations to cloud his conclusions. Even so, Form does not take full advantage of the information on French politics. The most noticeable absences are any prolonged discussions of the role of Catholicism in French politics and of the 1981 election of Mitterand.

These shortcomings will limit the utility of Form's book, although the scope and controversial nature of his thesis will mean that this book will become a valuable part of scholarly discourse.

JOHN F. ZIPP

*University of Wisconsin, Milwaukee*

**The Great Society and Its Legacy: Twenty Years of U.S. Social Policy.** Edited by Marshall Kaplan and Peggy Cuciti (Durham, NC: Duke University Press, 1986. viii, 252 p. $42.50, cloth; $17.95, paper).

Marshall Kaplan and Peggy Cuciti bring together a series of essays that treat the Great Society from many angles. The collection provides more than the standard discussion of the history and social and economic effects of the poverty programs. Attention is devoted also to the broader economic policy, the media coverage of the programs, the interplay of social science and public policy, and the new forms of governmental structures and processes that came about. Both advocates and critics are represented. In presenting the essays, the book attempts a sober evaluation of the programs and examination of what is to be learned from the experience.

The essays are themselves highly uneven in quality. Some of them, such as those by Bosworth on economic policy, Pechman on tax policy, Frieden and Sagalyn on inner-city revitalization, and Nathan on social science and the Great Society, constitute genuine attempts at probing deeper than the surface. These essays place the policies of the Great Society in a perspective offering readers fresh insight into the successes or limitations of the programs, the factors that shaped the programs' effects, and the understanding about public policy that resulted. Many of the others, unhappily, provide no analysis and offer little more than the standard fare that has been written and said many times before. Unfortunately, too, some of the foremost critics—people like Murray and Matusow—are not among the contributors at all. Indeed, in the end, Murray is simply dismissed out of hand with the presumption that his evidence and analysis are intentionally biased. This presumption neither flatters the authors nor strengthens their book.

Concurring with a lengthening list of students of public policy, Kaplan and Cuciti argue that the Great Society was responsible for more progress than is conventionally recognized. The antipoverty initiatives, they observe, made a significant contribution to the marked decline in poverty that took place from 1965 up until the latter 1970s. That decline did not result simply, or even mainly, from the growth of the private economy. While welfare constituted a disincentive to finding work for some people in some locales, the authors point out that the evidence does not demonstrate this generally to have been the case. Why, then, did so many people turn to governmental programs for help? The authors answer that the private sector of the economy was unable, on its own, to provide the numbers of jobs necessary at the pay necessary to lift undertrained and underskilled people out of poverty, let alone the elderly (who were largely outside the labor market) or the sick and the physically and mentally handicapped.

Kaplan and Cuciti leave unexplained why the private economy proved unable to lift people out of poverty following 1965 when it had shown itself to be very effective in doing just that from 1950 to 1965, before the Great Society. This is a key question. Was the Great Society at fault? Several of the contributors imply that possibly the economic and tax policies of the Great Society years may have been inadequate. Yet, most of the measures of real (after inflation) growth indicate that the economy grew even more rapidly in the 1970s than in the 1950s.

A more compelling explanation, one barely touched upon in the book, is rooted in the powerful demographic forces of the era: following 1964 a net increase of 30 million workers took place in the U.S. labor force. This staggering increase stemmed from the baby-boom generation and the increased entry of females into the job market. Working over-time just to generate the numbers of jobs necessary to absorb 30 million more workers, the economy could not in addition lift those on the bottom as it had done so ably in the 1950s. Without major programs of governmental assistance following 1965, disadvantaged workers would have been consigned to lives of unremitting poverty.

Notwithstanding the absence of an adequate explanation, Kaplan and Cuciti conclude that the Great Society's strategem to help the poor was, as a rule, on the right track. Will the government soon get back on that track? The authors observe that tax reductions coupled with increased defense expenditures and mushrooming expenditures by assistance programs for the nonpoor, all of which have strong political constituencies, make it unlikely.

JOHN E. SCHWARZ

*University of Arizona*

**Checks Unbalanced: The Quiet Side of Public Spending.** By Herman B. Leonard (New York: Basic Books, 1986. xii, 289 p. $18.95).

This is an interesting and intelligent examination of government programs and fiscal practices for which conventional budget accounting is inaccurate and misleading. Most of the attention to government's budget-control difficulties is directed toward the balance or imbalance between receipts and outlays. The purpose of Leonard's book is to analyze a number of fiscal problems that would not be resolved by balancing currently defined spending against currently defined receipts. And these fiscal problems, as Leonard points out, are not exclusively federal. State and local governments have managed, despite constitutional and other fiscal controls, to engage in fiscal schemes that are politically attractive but economically questionable.

What Leonard identifies as "quiet spending" ranges quite broadly. Included are, for example, the unfunded liabilities of public-pension systems and the social-security program. The pay-as-you-go funding policies used at all levels of government have created enormous unfunded liabilities for these programs. These funding policies have also served to cover up the actual costs of eligibility and benefit formulas. A similar obscurity regarding costs, Leonard argues, arises from the budgetary treatment of credit programs and tax subsidies. In both instances, substantial financial benefits are transferred without the scrutiny or controls that apply to direct spending.

There is at least some basic understanding of these types of programs and practices among budget and fiscal reformers. The inclusion of credit-program subsidies in the spending budget, for example, was advocated by a presidential budget commission two decades ago. Added to these conventional issues, however, are two relatively new and certainly unpublicized ones, the using up of capital assets through insufficient maintenance of public infrastructure and public-sector leasing. Both have been extensively used by public officials to avoid annual budget constraints, even though their long-term costs may be extremely high.

Tying this disparate set of fiscal difficulties together poses some problems. The standard Leonard uses to distinguish quiet spending is whether its financing scheme is controlled by the authorization-appropriations process. His point is that evasions of the appropriations process make it very difficult to enforce accountability. Since many of the programs financed in this fashion are financially complex and difficult to value on a current cost basis, one gets neither the oversight nor the programmatic competition that lends some discipline to direct-spending programs. Much of what Leonard analyzes, however, does run through the appropriations process. The actual costs, by contrast, are not necessarily controlled by that process. This is the case for annual appropriations ceilings on certain credit programs. The appropriations cap controls the volume, not the subsidy cost, of the program.

Leonard's conceptual distinction may, therefore, lack precision, but his general point about accountability being tied to costs is extremely important. His effort to bring these programs under a theoretical umbrella—using the principal-agent model to identify incentives for creating and maintaining quiet spending—likewise works better in some cases than in others. But, again, the general point that these programs have politically explicable commonalities is most persuasive.

Roughly two-thirds of this book is devoted to detailed studies of quiet-spending programs.

The remainder deals with various reforms that might raise the profile of quiet spending. An important issue here is how public-sector accounting might contribute toward a broader understanding of actual program costs, and Leonard's review of the problems and prospects is brisk and insightful, if perhaps a bit brief. Considerably greater discussion is devoted to how mechanical limits on spending—and especially the federal balanced-budget amendment—would not solve, and likely might exacerbate, quiet-spending evasions.

One of the great virtues of this book is the author's expertise. As a policy analyst, he understands the programs he is describing. As an economist, he can analyze fiscal and cost issues rigorously. And he uses both perspectives to illuminate the political process in a creative and knowledgeable fashion.

DENNIS S. IPPOLITO

*Southern Methodist University*

**The Political Hand: Policy Implementation and Youth Employment Programs.** By Martin A. Levin and Barbara Ferman (New York: Pergamon, 1985. xiv, 166 p. $19.50, cloth; $9.95, paper).

Since Pressman and Wildavsky in 1973 tagged implementation as a poorly understood and understudies policy stage, there has been a virtual outpouring of scholarly articles, books, and texts on the subject. In consequence, recently published contributions to what now is a fairly well-plowed field must fulfill greater expectations than in the past when each new work almost always added something valuable to the little that was known. Today, the critical reviewer of works on implementation looks for the following: (1) a conceptual and theoretical synthesis of the numerous fragmentary and particularistic rules and findings that exist in the literature; (2) the statement of an interesting and as yet undiscovered insight into the complex process of implementation; and (3) a clear exposition of the successes and failures of implementation in a particular substantive policy area that is useful to the students of that policy and for comparisons with implementation in other areas.

*The Political Hand: Policy Implementation and Youth Employment Programs* by Martin A. Levin and Barbara Ferman is a modest book that does not aspire to a general theory of the implementation process, which the authors believe exemplifies relationships too numerous and diverse to capture conceptually (p. 11, n. 7). Their task as they see it is less to create theory than to make the existing ideas more explicit. Scholars of implementation may differ as to the extent to which a satisfactory theory can, in fact, finally arise out of the weltering they have so far endured. Most will agree, however, that higher levels of theory should take top position on the research agenda, and some researchers have made strides in identifying general patterns or scenarios of implementation (see, for instance, Daniel A. Mazmanian and Paul A. Sabatier, *Implementation and Public Policy* (Glenview, IL: Scott, Foresman, 1981, chap. 8). From the perspective of a search for theory, Levin and Ferman's book is disappointing.

Judged from the perspective of the specific understandings added to unravelling the process of implementation, *The Political Hand* fares rather better. While clearly not a new notion, the role of implementation entrepreneurs (fixers or double agents) is clearly identified and imaginatively discussed as the political hand comparable to the unseen hand in economics that causes disorganized interests and only partially shared interests to converge in support of implementing policy. Despite their emphasis on the importance of leadership, Levin and Ferman do not mine their case studies for information about how such entrepreneurs can be recruited and nurtured. Instead they advise simple policy design since talented program executives are scarce and the disincentives to investment of political capital in implementation are strong.

Other lessons of the policy literature are clearly restated and reinforced by *The Political Hand.* Levin and Ferman note that while the reasons for policy-implementation failure are legion, the causes of policy-implementation success, achieved relatively well by the implementers of the youth-employment programs they studied, are more important and not sufficiently emphasized. These authors wisely observe that while implementation is important, it is not everything. Supposed ease of implementation of policy does not make up for design failures, and Levin and Ferman criticize a number of alternatives to existing policies that are sometimes suggested by more market-oriented policy analysts as not sufficiently targeted and not aimed at training. The study also reinforces the conclusion that by itself, adequate federal funding is not sufficient to achieve successful program implementation. More important is the preexistence of networks of human relations and rich organizational structure. Ironically in the case of youth employment, much of this crucial structure was built up during the Great Society period, which was accused of throwing money at problems without achieving results.

Only three of eight chapters and a short appendix contribute directly to substantive knowledge of implementation of the Youth Employment and Demonstrations Project Act of 1977. One chapter (chapter 4) concentrates on the characteristics of the youth unemployment problem. The main substantive treatment occurs in a chapter (chapter 5) that examines how each of nine separate selected programs worked and the degree of successful implementation. Data are presented in abbreviated narrative form and in an 11-page summary table that facilitates comparisons among the nine programs (pp. 62-73). A final chapter (chapter 8) provides recommendations to improve youth employment programs and critically examines some alternative program designs that appear to promise more effective implementation but are found flawed. While interesting to the student of implementation, substantive coverage may seem a bit thin for a reader whose principal interests are in the policy area.

Levin and Ferman have added a somewhat useful volume to what has become a rather crowded implementation literature that as yet lacks a theoretical synthesis.

HELEN M. INGRAM

*University of Arizona*

**Placing Parties in American Politics.** By David R. Mayhew (Princeton: Princeton University Press, 1986. 395 p. $38.50, cloth; $11.50, paper).

Describing U.S. parties has never been easy. In today's rapidly changing world, when we have difficulty deciding whether the parties are coming or going, relevant or irrelevant, we need more than ever to understand where they are coming from. In this book, David Mayhew has sought to isolate an aspect of those parties, one that he calls the "traditional party organization" (TPO) and to locate its existence during the 1960s. Such an organization by Mayhew's definition must (1) have substantial autonomy, that is, not be an arm of another group such as a union or interest group; (2) last a long time; (3) have hierarchic internal structures; (4) regularly try to bring about the nomination of candidates for a wide range of officies; and (5) rely primarily on material incentives to get people to work for them. Such parties have existed primarily where most of the patronage has existed, at the local level, although they may try to influence state politics as well. The classic machine is, of course, an example of a TPO, and one that sometimes could dominate an entire government. But Mayhew has cast his net more widely to encompass parties able to control only parts of the action.

How widespread were such organizations in the 1960s? The major part of the book is devoted to a state-by-state answer to that question. Aided by the outpouring of monographs and dissertations of the sixties and seventies, Mayhew is able to come to an informed judgement for each state. This section, and particularly its accompanying footnotes, stands as a most useful bibliographic essay on the condition of parties in the states. Mayhew impressionistically rates each state on a scale of one to five in terms of the relative importance of TPOs in their politics. He works from the assumption that if no scholar or journalist has written about them, they don't exist, a reasonable enough assumption given the United States' muckraking traditions. By the same token, I would have been a bit more skeptical in accepting accounts of their existence, particularly given the usefulness of the illusion of their existence in fending off unwanted competition.

Rooted as they are in the literature, the ratings will occasion few surprises. For those who thought that the Democrats of Chicago were the last outpost of this kind of material-based slating organization, Mayhew's survey provides an excellent antidote. The 13 "organization states," those rating a four or five, form a region running from Rhode Island

and Connecticut down through the Middle Atlantic states to Maryland and ranging westward through Illinois to Missouri. Louisiana gets the only three in the nation, thus saving the five-point character of the scale. Five southern states rate a two, as does New Mexico in the west. The remaining 30 states receive a one, indicating no evidence of "traditional" organization.

What difference does it make for the policies of a state? Mayhew correlates his TPO scale with measures of the states' expenditures related to personal income to show that states with traditional parties have had significantly smaller public economies in relation to income than states without such parties. That the correlation exists is clear enough, but given the regional concentration of the organization states, their relative age, and population density, I am not convinced that the causes lie simply in the character of their local parties.

The principal deviant here in New York, a five on the TPO scale, yet a state with a large public economy. Mayhew attributes its odd behavior to its strong reform tradition, and the fact that the state"specialized in executives with a bent for aggrandizing government." That most of those executives ended up as national party leaders in both parties is ignored.

Herein lies the major problem with this "bottoms-up" approach to party organization. It cannot recognize that these are, and always have been, national organizations into which state and local organizations are linked more or less tightly. Mayhew, of course, recognizes that other sorts of party organizations exist (usually he refers to them as "candidate organizations"). Yet it is clear from his title, *Placing Parties*, as well as from his approach, that only the traditional party is the real thing. Yet, as his figures show, the last time the "organization" states encompassed as much as half the population of the United States was in the 1880s, and that proportion has been going down ever since. Mayhew's parties are traditional not because they represent the way in which U.S. parties have organized themselves, but only in the sense that U.S. scholarly tradition has deemed them the true U.S. party. It is this tradition that insists that U.S. parties have no national organization and that the center of gravity of power in these parties lie at the local level. If we are truly to "place" parties in U.S. politics, we must use concepts that come closer to the real world. It is ironic that students of parties should persist in constricting their views of party organization to those elements that resemble well-defined structures like businesses, at a time when organization theorists have come to recognize the inapplicability of such simple hierarchic models to these organizations.

JOSEPH A. SCHLESINGER

*Michigan State University*

**Chicanos, Catholicism, and Political Ideology.** By Lawrence J. Mosqueda (Lanham, MD: University Press of America, 1986. vii, 219 p. $24.50, cloth; $12.75, paper).

The significant growth of the Chicano population has fueled wide interest in their political orientations. Politicians, political pundits, and Chicano activists appear to be equally engrossed with the question of how the demographic ascendance of Chicanos might alter the nature of local and national politics. In this light, the appearance of *Chicanos, Catholicism, and Political Ideology* is certainly timely: the potential readership is large and eager. The ability of this work to satisfy the curiosity of that readership, of course, is another question.

The body of this small volume was the author's dissertation and, unfortunately, its content is typical of many degree-conferring projects. To be specific, it is narrow in scope, formula-bound in structure, short on data, and more descriptive than analytic. But given its origin, the blame for these predictable faults probably rests more heavily on the publishers. They should have encouraged more additions and revisions.

Data were gathered between 1977 and 1978 from 35 Chicano informants via an open-ended interview instrument. The respondents were either community leaders, academics, or clergy. The book tries to address two points directly: (1) Is the Catholic church a mainstay in the everyday lives of Chicanos? and (2) Does the church predispose Chicanos to embrace conservative political ideologies?

Mosqueda concludes that Catholicism is not an integral part of Chicanos' socialization process. Chicanos readily express being Catholic as part of their subjective identity but they lack commitment to religious obligations and often diverge from church doctrine. All this may be true, but the evidence presented is not very convincing. The bulk of his argument is based on rates of church attendance during the sixties.

Is the church a conservative factor among Chicanos? I don't think the book ever really tackles this issue head on. Those who believe that Chicanos are naturally inclined to be political conservatives because they are Catholics, are strongly warned to reconsider their views. However, if you have ever wondered what the relative importance of Catholicism is on formulating Chicano political ideologies, you will have to look elsehwhere for insights.

From the verbatim responses that were printed, it is clear that Chicanos who had attained a high level of religious devoutness had also clashed with the church or their own beliefs when they adopted liberal political postures. And the book describes the clashes between the church and the United Farmworkers Union and the conflicts that arose between Los Angeles's Cardinal James McIntyre and Catolicos por la Raza. Both of these events corroborate the respondents' collective opinion that the church has been on the conservative end of the political spectrum and it will try to act as an agent of social control. But it is difficult to go meaningfully beyond this essentially historical observation.

The book's major drawback is that it never definitively states what have been the normative parameters of Chicano political expression. Simply to imply that Chicano activists have rarely ventured further than adopting a reformist stance just isn't a sufficient foundation for addressing the book's major research question. How can we discuss whether the church has significantly shaped the political ideology of Chicanos if we can't even describe the general dispersion and centrality of any specific political attitudes or behaviors?

Survey data collected in the 1980s suggest that Chicanos oppose gun control, favor the death penalty, and don't think blacks need federal intervention. Chicanos also favored the Equal Rights Amendment and are prochoice with regard to abortion. And they voted overwhelmingly for Senator Ted Kennedy in the 1980 presidential primary. What role did Catholicism play? I am sure that Dr. Mosqueda is now effectively addressing that issue.

PHILIP GARCIA

*California State University*

**Judging Credentials: Nonlawyer Judges and the Politics of Professionalism.** By Doris Marie Provine (Chicago: University of Chicago Press, 1986. xvii, 248 p. $30.00, cloth; $13.95, paper).

*Judging Credentials* is about the professionalization of the U.S. judiciary. Its focus is upon the credentials (presence or absence of a law degree) of judges sitting in the lowliest of U.S. courts, those hearing traffic and other minor criminal and civil matters. The author, a political scientist and lawyer who once served as a judge in a rural New York court, addresses the central issue of the *competence* of lay judges from historical, contemporary, and empirical vantage points. Provine's thesis is that lay judges perform as well as lawyer judges and, in so doing, confer a unique democratizing influence on the justice system.

The book has two major parts. The first is a historical view of the development of an all-lawyer judiciary in the United States and subsequent efforts by the organized bar and court reformers to extend this to the lowest courts. The book's second part addresses, with data collected from New York State, the attitudes and performance of lay and lawyer judges.

The historical view is superb. In drawing upon various disciplines, the research is thorough and highly eclectic. Nevertheless, readers may find it interesting to compare Provine's images of lawyers in the early United States with those of Robert Ferguson in *Law and Letters in American Culture* (Cambridge: Harvard University Press, 1984). If it seems that the two are speaking of an entirely different species (contrast Provine's lawyer as job hunter with Ferguson's lawyer as leader of the literati), perhaps it is because the legal profession was diverse and stratified then as now.

The New York-based data, consisting of a mail survey to lawyer and lay judges as well as observations and interviews, have been collected with rigor and a theoretical sense of purpose. Several states, though, could profitably have been studied, given that 43 still utilize lay judges. More problematic is Provine's interpretation of her data. She finds that: lay judges are more likely to penalize offenders who originally plead not guilty (p. 100); more lay than lawyer judges (62% v. 45%) are critical of the due-process revolution (p. 101); and, perhaps most telling of all, a sizeable group (20%) of lay judges themselves feel inferior to lawyer judges (p. 163). Nevertheless, Provine concludes that her data "failed to expose any *major* differences between lay and lawyer judges" (p. 120, italics added). In my view, she does not adequately address questions about how large differences must be to have theoretical or legal significance, and whose frame of reference—the lawyer's (oriented to "worst-case" abuses) or the social scientist's (viewing judges in the aggregate)—should be used in evaluating differences.

Nor does Provine grapple with the policy implications of these differences or worst-case abuses. She fully describes the Supreme Court's refusal *(North v. Russell,* 1976) to ban the states' use of lay judges but *not* how the judicial abuses of *North v. Russell* are to be avoided in the future. Her own data (59% of New York's lay judges had no education beyond high school) provide little reassurance to those who believe that college-level education contributes substantially to a civic literacy desirable for lay (and lawyer) judging.

By contrast, the book is at its best in examining the broad picture. Provine ably connects her empirical data to the larger theoretical concerns. Along the way, readers will find relevant discussions of the organized bar, alternative dispute-resolution forums, and citizen participation on juries. Particularly interesting is the discussion of lay judging in other countries, not only in Britain but in Western and Eastern Europe.

*Judging Credentials,* despite some reservations about its empirical features, is an excellent scholarly contribution, not only to the literature on courts but to the study of professionalism in U.S. culture. The book is also well organized, concise, and literate and could be effectively used in upper-division and graduate law-related courses in political science, sociology, and history.

JOHN PAUL RYAN

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**Rural Public Administration: Problems and Prospects.** Edited by Jim Seroka (Westport, CT: Greenwood, 1986. 208 p. $37.50).

The Seroka volume contains introductory and concluding essays by the editor, and a collection of 10 previously unpublished "research reports" on a diversity of subjects concerning public administration in rural areas. The results of the 1980 census indicated evidence of a "rural renaissance" of sorts whereby non-Standard Metropolitan Statistical Areas grew much more rapidly than urban areas for the first time in this century. As awareness of this rural rebirth spread, considerable scholarly attention was directed toward rural government. Symptomatic of this awakening of attention among public administrationists is the creation of a new section within the American Society for Public Administration devoted to "rural public administration." Together with the publication of Beth Honadle's *Public Administration in Rural Areas and Small Jurisdictions: A Guide to Literature* (New York: Garland, 1983), this volume represents the first swell in what promises to be a tidal wave of new scholarship on rural governments and their administrators.

The common sentiment underlying the Seroka volume, the Honadle bibliographic work, and these several specialized programs is the view that established Social Science and Public Administration are dominated by urban concerns. In his introductory essay, Seroka states the view thus: "The primary objectives of this collection are to highlight and to underscore the research opportunities in the field of rural public administration, and to reassert the need to study rural governmental units from a fresh perspective that is conceptually and practically autonomous from urban analysis" (p. 3, italics added). In his concluding essay, Seroka goes on to argue that "it is unfortunate but true that many of the scholars involved in the field find themselves as professionally isolated as many of the rural communities they study. No single professional association speaks to the concern of these interests, no journal regularly publishes rural public administration findings, and scholars in the subfield must fight for attention against the strong undercurrent of a pro-urban bias of many general public administration and social science journals whose editors and reviewers pronounce that rural concerns are of limited or marginal concern to their readership" (p. 186). Given the advent of significant population growth in many rural areas, the lack of non-urban-oriented analytical perspectives, and the supposed hostility of established disciplines, exactly what is to be learned from this uncensored collection of essays and empirical studies on rural public administration?

Brief summaries of some contributions to the Seroka volume will give the reader a view of the main issues concerning the self-styled victims of an allegedly biased, prourban scholarly community. Frank Bryan's essay concerns the problem of proper definition for the key term *rural*—maintaining that nonmetropolitan or small town-oriented definitions are inferior to spatial/population dispersion-based definitions. Bryan argues that the rural area is one of dispersed population and personal relations, while the urban area is one of dense population and anonymous interpersonal relationships. These differences, in turn, create very different cultural environments, giving rise to unique operating conditions for administrators in urban and rural settings. Seroka's contribution notes the existence of four very different types of rural areas based upon the combination of economic and population trends: (1) areas that are growing in population and improving in economic climate, (2) those that are declining in both, and (3, 4) those with growth in one area and decay in the other. Very different administrative conditions tend to occur in each of these types of areas. Cal Clark and his colleagues, Alvin Sokolow, Edward B. Lewis, and Keith Snavely—in separate chapters dealing with fiscal behavior, governing-board conduct, the behavior of chief county administrators, and the operation of personnel systems—indicate that the conventional wisdom on how governments and administrators behave seldom applies in a simple way to rural phenomena. Moreover, in the area of economic development, Lewis G. Bender and Thaddeus C. Zolty do an excellent job of setting forth the "crisis oriented" nature of governmental change and adaptation in those rural areas where development is sorely needed. Finally, Ken Hibbeln's analysis of the consequences of the demise of the doctrine of sovereign immunity at the hands of the courts indicates how rural jurisdictions are disproportionately disadvantaged by such developments.

This brief overview serves to indicate how many areas of rural public-administration research are indeed of considerable interest—both for their obvious pertinence to those working and researching in the rural-administration field, and for their interest to students of urban or local government generally. Our generalizations about political and administrative behavior established in metropolitan centers ought by all means to be tested in rural settings, thereby providing for more adequately tested general theories of governance and more fully developed information on the character of rural government. While this volume does not provide the firm basis for a comprehensive approach to such research, it does indeed serve as a stout early step in this direction.

NICHOLAS P. LOVRICH, JR.

*Washington State University*

**TVA and the Tellico Dam 1936-1979: A Bureaucratic Crisis in Post-Industrial America.** By William Bruce Wheeler and Michael J. McDonald (Knoxville: University of Tennessee Press, 1986. xii, 290 p. $34.50).

Two historians have written a book of value for political scientists in their portrait of an organization in search of a mission. The missions of the Tennessee Valley Authority (TVA) were ambiguous from the beginning. President Roosevelt talked of a broad mandate for regional development, but the statute assigned specific responsibilities to build dams, create and distribute electric power, improve navigation and flood control, and assist agriculture, in part by manufacturing fertilizer. The broad mission of regional development was covered only in the authorization of planning studies. The great achievements of the 1930s and 1940s were the production of electric power, navigation and flood control, and improvement in farming through new fertilizers and technical assistance, as well as a major contribution to the production of electric power for the war effort, including the mysterious facility at Oak Ridge.

But the ideal of a multipurpose authority engaged in regional development did not die. It was appropriated by David Lilienthal and his successor as chairman, Gordon Clapp, as part of the "myth" of organizational purpose that kept the Washington politicians and bureaucracies at arms length. Furthermore, it was believed by many dedicated people in the organization. However, once the dams were built, to be followed by steam plants, and once the river was straightened and agriculture assisted, what contributions was the TVA to make to regional development?

This book is the story of the search for an answer to that fundamental question. The Tellico Dam, originally conceived in 1959, was to be the expression of a new TVA mission of regional economic development. The dam, to be built on the Little Tennessee River, was not to be justified in terms of the traditional goals of power generation, navigation, or flood control. The cost-benefit ratios fell short of accepted criteria. It was to generate economic development through recreation, enhanced land values around the reservoir, and industrial development.

Tellico's author was Aubrey Wagner, TVA general manager, board member and, from 1962 to 1977 chairman of the board. Wheeler and McDonald depict Wagner as using the Tellico idea as a catalyst to unify the TVA as an organization. A deep division had developed between the dominant office of power and the smaller resource-development programs of navigation, forestry, community development, planning, and agriculture. Wagner hoped that new economic-development projects, like Tellico, would unite the organization, restore the vitality of the "myth" of a multipurpose mission, and create external support for TVA.

But the story after 1959 is of a disaster for TVA. Local groups in the Tellico region were organized by TVA to support the project, but public opposition from fish and wildlife groups and farmers opposed to flooding land soon developed. The agriculture, forestry, and fishery staffs within TVA were opposed and were overridden by Wagner. Favorable cost-benefit ratios eluded the TVA planners, and there was much manipulation to produce acceptable figures on the basis of shaky assumptions. Just as Congress authorized the appropriation in the midsixties, the environmentalists launched an open attack, insisting that TVA file an environmental-impact statement. Eventually, the Environmental Defense Fund won an injunction in federal court that stopped construction in 1972, and the Governor of Tennessee attacked the project.

TVA might have outlasted all these opponents had not the tiny snail darter been discovered in the river in 1973, causing the U.S. Fish and Wildlife Service to declare the river a critical habitat for that "endangered species." The federal court eventually ruled that the dam could not be completed unless Congress exempted it from the Endangered Species Act, which Congress did, through the influence of Senator Howard Baker in 1978. The TVA had won, but it was a hollow victory. Wagner was gone, succeeded by S. David Freeman, an environmentalist who used the Tellico fiasco as an opening to redirect TVA's mission along environmentalist lines.

The authors believe that TVA ruined its reputation with Tellico. It stonewalled internal dissidents and external opponents. It revealed bureaucratic anger at opposition of any kind. And there was a failure to see that the mission of economic development through water projects was outdated before inception.

Aside from a good story well told, what is here of special value for political scientists? There is confirmation of the insight that TVA could perform, quite well, tasks for which it had specific statutory authority. But the general task of regional development would always elude it. Second, there is confirmation of Philip Selznick's description of two kinds of "cooptation" as set forth in *TVA and the Grass Roots* (Berkeley: University of California Press, 1949). The first kind of cooptation was the surrender of the agricultural-assistance program to the valley agriculture colleges and the extension service. TVA permitted these groups to dominate the agricultural mission. The second kind of cooptation, less developed by Selznick, was TVA dominance of community groups through formal modes of representation. The boards of municipal power distributors, for example, were set up by TVA to be politically weak and unable to challenge TVA decisions about rates and services. TVA actions to create "grass-roots" support for Tellico were in this latter mode. Third, this is a case study of a bureaucracy in search of a mission that will revitalize it. TVA could have continued as a progressive power company. But the ideal of multipurpose regional development was too strong and had to be fulfilled. Ideals explain Wagner's behavior as much as do bureaucratic incentives.

ERWIN C. HARGROVE

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# COMPARATIVE POLITICS

**The State and Social Transformation in Tunisia and Libya, 1830-1980.** By Lisa Anderson (Princeton: Princeton University Press, 1986. xxiv, 325 p. $35.00).

**Algeria, the Revolution Institutionalized.** By John P. Entelis (Boulder, CO: Westview, 1986. xii, 239 p. $24.00).

**Egyptian Politics under Sadat: The Post-Populist Development of an Authoritarian-Modernizing State.** By Raymond A. Hinnebusch, Jr. (Cambridge: Cambridge University Press, 1985. viii, 322 p. $35.50).

Anyone not directly interested in northern Africa will learn from these three fine books much of great value on issues central to political science—modernization and the autonomy of the state. Specialists will also benefit from studies so thorough, reliably accurate, and critical. All three books, precisely because of their high quality, also serve to clarify the current limits of theoretical perspectives in the field.

Lisa Anderson admirably synthesizes 150 years of experience in two neighboring countries to show how the persistent growth of a bureaucratic state acted as an autonomous force in creating a modern political organization and social structure in Tunisia, while the repeated dismantling or neglect of bureaucratic state structures shaped a radically different Libya. In both instances the state is examined as playing an independent causal role. In contrast to its role in Western Europe, the state here alters and dominates the structure of society. In its absence, as in Libya, no other structure appears able to mobilize, develop, and transfer resources effectively within society. Marxist and Weberian perspectives, field research, and sources in French, Italian, Arabic, and English inform this excellent work.

In the studies by Hinnebusch and Entelis, the state is also clearly the dominant force, but the principal development in the change from Nasser to Sadat in Egypt is the rise of the commercial Egyptian bourgeoisie and the United States as principal allies of what Hinnebusch calls a "presidential monarchy" (p. 89). That class yields some of its political freedom to the state in return for special economic favors; that state pays this state for political favors. Considering that Egypt has been accustomed for millennia to a bureaucratic state but that Nasser structured his "authoritarian, populist, and modernizing" regime and pursued policies quite differently from his successor, Sadat, Hinnebusch does not pause for long to argue for the autonomous role of the state. Instead, he analyzes the changing role of an authoritarian presidency in its total context: an apex of power reinforced and hindered by a huge ossified bureaucracy, capable of rewarding or diminishing individuals in intraelite conflicts, repressing counterelites and masses alike, and implementing policies that increase the gap economically and culturally between the elite and the great majority. Hinnebusch examines the toll of such rule in fascinating and vital detail with respect to executive power, parties, parliament, press, education, courts, social classes, cultural traditions, and major foreign and domestic issues without ever sounding merely encyclopedic. His book draws upon four years of Egyptian field experience and a keenly reflective and caring mind.

A state effectively controlling all of Algeria emerged for the first time only in the mid-nineteenth century. That state, legally an integral part of France until 1962, inflicted enormous and prolonged suffering on Algeria's Muslim population until an eight-year war, producing a million casualties, finally brought independence and, among a predominantly poor and illiterate population, much internal dissension. Twenty-five years after independence, Entelis concludes, Algeria "has achieved a remarkable degree of political stability and economic growth" (p. 1). "Efficiency, accountability, and productivity are the hallmarks of the new Algeria" (p. 66).

Shall we thank the state? "The revolution institutionalized," Entelis tells us in a most knowledgeable and lucid review of Algeria's political economy and political dynamics, is constituted as "sultanic" rule—"a tightly closed circle of power around a single leader . . . independent of all groups having an autonomous base of power, whether these be institutional, political, administrative, or social" (p. 162). In Algeria, the sultan relies upon an apolitical technocratic elite produced by an educational system "directed more at producing administrators than politicians, doers than thinkers, imitators than inventors, technicians than scientists, and organizational men rather than independently minded and creative decision-makers" (p. 93). This is not surprising, as the military remains decisive in selecting and sustaining the sultan.

Entelis has produced the best book in English analyzing three decades of Algerian development from bitter repression to prolonged warfare to misery and dissension to stability. His conclusion, however, is cautiously particularistic.

For the past half century, the main opposition in all Muslim countries to the growth of such states have been neo-Islamic totalitarian movements composed of people who feel entirely left out and cannot imagine being at home in such a world. Do political scientists have no fundamental questions of their own about the value of this kind of state? Anderson is well aware of what she has left out of her analysis: "the tremendous agony" (p. 12) of the subjects of the "rapacious" (p. 115) native architects of the state who made their people "destitute and dependent" (p. 115). She adds that she did not plan to speak of hopes, desires, and cultural identity in her effort "to discover the pattern and processes of structural transformation in the periphery" of the West (p. 12). All three authors are personally quite sensitive to the suffering of the people and do not fail to report the costs of state formation. What is missing in political science now are theories that allow us systematically to pay attention to, and integrate into our analysis, that human agony, even as we examine the modern bureaucratic state becoming the predominant force in much of the world.

We concentrate on demonstrating that great revolutions produce stronger states than before; that the state can be an autonomous force in harnessing and altering power, resources, and social structures; that different forces can capture or check that state. These are valuable demonstrations. But who is the state and what are the fundamental tasks of people in society in the modern age? If the state is *that part of society that claims a legitimate* monopoly on coercion and if its bureaucracy deals either impersonally or personally with others depending upon the interests and corruptibility of personal rule at the top, then the state is a very costly instrument of order, unable to accomplish what counts. What counts is helping people to establish a new community and a new and just way of life when the old community and its way of life are dissolving and many of the forces of order have become repressive and unjust. At present, we lack theoretical grounds for criticizing power even when, as human beings, we do not fail to be touched, and when, as analysts, we feel compelled, at least for prudential and pragmatic reasons, to speak out.

MANFRED HALPERN

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**The State and Capital Accumulation in Latin America: Brazil, Chile, Mexico.** Edited by Christian Anglade and Carlos Fortin (Pittsburgh: University of Pittsburgh Press, 1985. xiii, 254 p. $23.95).

Missing in studies of Latin American politics has been an empirical and theoretical focus on the state and the impact of state policy on capitalist accumulation. This first of a projected two volumes of country studies helps to fill the gap. Its origins date to a 1978 Conference of the Political Studies Association of the United Kingdom, but the original papers have been updated and substantially revised to reflect the collapse in the eighties of the export-oriented models that had been adopted by many Latin American countries in the preceding decade, the increasing dependence on foreign financial capital, and the important role of the state in direct production and capitalist accumulation.

This initial volume is organized around four cogent and carefully researched essays. The introduction by Christian Anglade and Carlos Fortin offers a conceptual and historical overview of the state and capital accumulation in Latin America. Their essay examines the early and present debates on capital accumulation in the capitalist periphery, including the arguments of W. A. Lewis and Albert Hirschman, among liberal economists, and Paul Baran, Bill Warren, and others, among Marxist economists and social scientists. Acknowledging that Marxist theory of the state is itself undergoing a crisis, the authors turn to Marxism as their most promising conceptual framework. They identify two meanings of the state: (1) the domination of classes and fractions of classes over society and (2) the institutions and personnel through which class domination is expressed. In this context, they review the types and forms of state, forms of regime, functions of the capitalist state, roles of the state and bases of domination, and relative autonomy of the state. They relate these conceptual concerns to a retrospective historical overview of the state and capitalist accumulation in Latin America, with attention to the primary-export state, the populist state, the crisis of the populist state, and the integration of the state into the world economy.

The essay of Anglade on Brazil looks at the economic model in the post-1964 coup period by tracing its origins to the import substitution-populist period during the Kubitschek administration of 1956 to 1961. He analyzes the functions of the state in capitalist accumulation after 1964 and argues that in Brazil "the insufficiency of domestic savings and the absence of a domestic control over the process of capital accumulation are inherent in the model of growth which was adopted . . . they are the ultimate causes of the crisis and a structural obstacle to the capitalist development of the country" (p. 53). He demonstrates through detailed data and analysis that the Brazilian model of accumulation suffered from inherent contradictions that eventually brought about "its virtual collapse, instead of allowing accumulation to take place at a higher level" (p. 127). He concludes that the Brazilian experience increased "the externality of the process of capital accumulation" and failed to promote sustained growth because of "the pattern of gross inequality in the distribution of wealth and income" (p. 130).

In his examination of the post-1973 Chilean political economy of repressive monetarism, Fortin first identifies its background in the "compromise" state during the 1940-64 period, in the reformist model of accumulation during 1964-70, and in the socialist model of accumulation under Allende during 1971-73. He breaks the post-1973 period into three political and three economic stages, then looks at the impact of state intervention on public revenue and expenditure, the reorganization of the financial system, and the management of the external sector. He identifies contradictions of the Chilean model and relates the ensuing economic crisis to class conflict: "A financial bourgeoisie was able to hegemonise a power bloc composed of all fractions of the bourgeoisie inasmuch as it seemed to offer the only way to avoid a repetition of the Allende threat. Once in power it started implementing a de-accumulation model which led it into conflict with the other fractions and allowed for a re-emergence of the popular sectors" (p. 196).

E. V. K. Fitzgerald provides the third case study, with attention to the state and capital accumulation in Mexico from 1940 to 1982. He examines constraints on the relative autonomy of the Mexican state, the patterns of capitalist accumulation, and the fiscal crisis of the state, based on "the imbalance between the acceleration in state accumulation and the inadequacy of its own surplus to finance it" (p. 227). He attributes the fiscal crisis to three trends in the accumulation model: "the exhaustion of the post-revolutionary industrialisation model, requiring increased state investment to take the place of private investors," "the opposition of the private sector . . . to greater state control over the surplus," and "the inevitable consequence of attempting to reconcile these two trends by foreign borrowing" (p. 229).

The themes of state and capitalist accumulation are carefully tied to the analysis of the three case studies, which gives this book coherence and focus. The authors set forth clearly their theoretical framework and enhance their discussion with the results of careful research on policy questions relating to the state, economic data on capitalist accumulation, and analysis of underlying contradictions in the economy and in political and social conflict. Andrade's essay on Brazil is the most exhaustive and detailed, with its political economic analysis focused on the implications of state policy. Fortin's essay on Chile is more readable, involves less statistical analysis, and connects effectively with the political and social reality, while Fitzgerald's piece on Mexico is lucid and useful, although brief and exploratory. Overall, this volume represents a serious, scholarly, pioneering effort and a contribution of considerable theoretical and empirical importance.

RONALD H. CHILCOTE

*University of California, Riverside*

**Zia's Pakistan: Politics and Stability in a Frontline State.** Edited by Craig Baxter (Boulder, CO: Westview, 1985. ix, 122 p. $6.50).

**Islam, Politics, and the State: The Pakistan Experience.** Edited by Mohammed Asghar Khan (London: Zed Books, 1985. xv, 281 p. $29.50, cloth; $9.95, paper).

**The Military and Politics in Pakistan 1947-1985.** By Hasan-Askari Rizvi (Lahore: Progressive, 1986. xiv, 355 p. $25.00).

Pakistan is presently undergoing a considerable amount of political change and uncertainty. Elections in February 1985 were followed by the establishment of civilian government, the formal ending of martial law (on 30 December 1985), and steps toward the restoration of legal political parties. The new civilian regime is fragile and subject to political pressure and protest. Meanwhile, the military remains a key factor, with General Zia ul-Haq continuing to hold ultimate power as president of the country and chief of the army staff.

The three books under review provide a diverse and complementary set of perspectives on these various developments. Rizvi's book, an update of editions published in 1974 and 1976, provides an excellent review of military involvement in Pakistani politics throughout the country's four decades of independence. Of particular value is a new chapter ("The Return of the Military," pp. 218-53), which is the best succinct review yet available of the history of General Zia's martial-law regime and subsequent developments. Rizvi traces the events surrounding the coup d'etat of 5 July 1977, the changing goals of the military rulers, their relations with civilian political parties and religious groups, their infiltration of the bureaucracy, and the recent moves to "civilianize" the system. Rizvi concludes (p. 253) that the new civilian government will need to broaden its base of support or remain permanently dependent upon the military for its survival.

The Baxter volume is a collection of seven brief but perceptive essays, produced under contract with the U.S. State Department. The authors attempt to assess Pakistan's potential for change by looking at several key groups within the society. Robert LaPorte (urban groups), Charles Kennedy (rural groups), and John Adams (the economy) all suggest that political discontent has been kept within manageable limits because of the "perception of opportunity" (p. 11) that has accompanied economic growth and the emigration of Pakistani labor to neighboring Middle East nations. Rodney Jones, in reviewing military and security issues, notes further that Zia's regime has depended less on coercion (although there has been more of that than any of these essays imply) than on cooptation. The Jones essay makes a good companion piece to Rizvi's book and to Stephen Cohen's excellent work, *The Pakistan Army* [see below, p. 652]. Grant Farr's essay on the Afghan refugees in Pakistan notes that the refugee issue has thus far been handled very well but warns that the potential for problems will increase with the passage of time. Finally, Craig Baxter concludes with speculation on the future legitimacy of the changing regime. He outlines four possible scenarios, of which he regards "a civilian facade with an active military role" or, alternatively, a "return to military rule" (pp. 118-19) as most likely.

*Islam, Politics, and the State* is also an edited volume, but the focus is more specifically on the Islamization issue and the tone is considerably more critical and polemical than that of the other two volumes under review. Most of the contributors are Pakistanis.

The chapters by Pervez Amirali and Abdul Hoodbhoy on the political distortions of history writing, by Hoodbhoy on the decline of science in the face of religious conservatism, and by Feroz Ahmed on Sindhi nationalism are all interesting because of their treatment of relatively fresh and timely topics. Too many of the contributions, however, dwell much too long on well-worn themes such as the nature of the Pakistan movement and the thoughts of Pakistan's founder, Mohammad Ali Jinnah.

The editor of this collection, retired Air Marshal M. Asghar Khan, is leader of the Tehrik-i-Istiqlal, one of the political parties outlawed by Zia in 1979. Asghar Khan and the Tehrik are generally considered to be only moderately left of center in the Pakistani political spectrum, while most of the contributors to this volume are more radically Marxist. The collaborative effort might therefore be seen as a marriage of convenience, with Asghar making a bid to compete with Benazir Bhutto and her Pakistan People's Party (PPP) for the support of leftist opponents of the present regime.

The continuing commitment of U.S. military and economic assistance to Pakistan, uncertainties concerning the prospects for a negotiated settlement to the Afghan war, and the recent resurgence of antigovernment protest in Pakistan all underline the value of these three works to the general reader as well as the South Asia specialist.

WILLIAM L. RICHTER

*University of Kansas*

**Labor in Latin America: Comparative Essays on Chile, Argentina, Venezuela, and Colombia.** By Charles Bergquist (Stanford, CA: Stanford University Press, 1986. x, 397 p. $39.50, cloth; $14.95, paper).

This stimulating book sets forth the Bergquist thesis, which should spawn lively debate among social scientists working on Latin America and other parts of the Third World. Dependency and world-system theorists long ago established, at the very least, that the export sector decisively influenced the development of these countries. A few years ago, the new social historians shifted scholarly attention from international structures down to the ground-level experiences of the unpublished poor. Drawing selectively from both those approaches, Charles Bergquist argues that export workers largely determined the economic and political trajectory of modern Latin America.

In his elaboration of this original and powerful perspective, the author takes into account multiple variables and variations. Without being mechanistic, he distinguishes between two polar types. Chilean nitrate miners and Venezuelan petroleum workers tended toward leftist movements growing out of a foreign-owned, capital-intensive, export enclave which contributed little to national development. By contrast, Argentine meat-packers and Colombian coffee producers chose more conservative political options reflecting domestically owned export systems whose multiplier effects furthered the growth of national capitalism. Within that dichotomy, Bergquist traces the subtleties and complexities of individual cases, noting such mediating factors as party systems and cultural traditions.

Even those skeptical of the interpretive framework will welcome the rich national histories crafted out of extensive field research. This book provides bountiful information and insights on these export sectors, including production trends, workplace organization, origins of owners and laborers, and linkages to national development. The most original material concerns Argentina and Colombia. In the former country, the early success of livestock and cereal exports undercut labor and the Left, which allowed Peronism to dominate national politics. Similar weaknesses in unions and progressive parties in Colombia favored the hegemony of traditional conservatives and liberals, partly because the character of coffee production satisfied many workers with small family farms. Since oil revenues could pay for social reforms, Venezuelan petroleum workers also supported a liberal capitalist system, although they spearheaded the democratization of politics and the nationalization of oil. The most radical labor movement arose in Chile, in reaction to the failure of nitrate and copper exports to fuel national development. These insights fully justify Bergquist's call for greater scholarly attention to export workers.

Like most revisionist theses, this argument is somewhat oversold, as the author concedes. It will require more explicit elaboration of the causal links and more testing in countries with diverse export and political structures. Bergquist's admirable love for his subject matter also leads him, at times, to exaggerate working-class militance. For example, Chilean copper miners struck against President Salvador Allende and then cooperated with President Augusto Pinochet until the economic collapse of 1981, suggesting a fairly pragmatic orientation. Their crucial role in reactivating the democratic opposition, however, points to the validity of Bergquist's focus. The author's sympathies may also be causing him to overstate the likelihood of future progressive triumphs by the proletariat. Whether or not Bergquist's optimistic vision proves a guide to the future, his daring analyses have certainly illuminated the past.

PAUL W. DRAKE

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**The Pakistan Army.** By Stephen P. Cohen (Berkeley: University of California Press, 1984. ix, 177 p. $27.50).

This is not a book about the romance of the Khyber Pass or hardy Pathan tribesmen and simple, valiant *jawans*—nor is it an expose of power-crazed generals trampling the tender shoots of Pakistani democracy. Rather, it is a realistic study of the Pakistani officer corps, their role in Pakistani politics, and the strategic dilemmas that they face. There are valuable brief surveys of the basic sociology of the Pakistan army as well as an exemplary, concise section on where Pakistan stands in the world militarily and the constraints that face a middle power in a pitiable geographic location. Cohen does not, however, delve into the arcana of organizational tables and battlefield tactics. His emphasis is on the role of the army in Pakistani politics and its strategic dilemmas. It is very much the work of a political scientist rather than a military historian, and appropriately so, since the Pakistan Army has assumed such an intensely political role.

A different book might have stressed the admirable qualities of the Pakistani soldier facing discouraging odds, but as a political instrument and even in its more directly military role, the Pakistani Army has come up against challenges that exceed its capabilities and Cohen does not hide the fact. The author, whose knowledge of the armies of former British India ia unequalled among U.S. academics, has enjoyed extensive access to the Pakistan military and is obviously on close terms with many Pakistani officers (including President Zia ul-Haq). Yet this book has been banned in Pakistan—and that tells us something about the honesty of the book as well as the insecurity of the Pakistani military, for Cohen is basically a sympathetic and concerned, but objective, student of the subject who takes seriously his self-appointed role of "friendly critic."

The Pakistani army faces daunting problems. The strategic threats posed by India and the Soviet Union are overwhelming; its allies are either unreliable (the United States) or themselves militarily backward (China). The social fabric of Pakistan is split along serious fault lines—between the dominant Punjabis and the other ethnic groups; and between modernizers and Islamic fundamentalists—and the army reflects these tensions. It is perceived as a primarily Punjabi institution. It is torn between adherence to foreign models that estrange it from its own context and acceptance of indigenous values that shade readily into corruption. In the face of abominable performance by Pakistani politicians (most recently Z. A. Bhutto) the army has burdened itself with the task of governing a large and diverse country, but it lacks even the political skills of the discredited civilians. While the Pakistani people enthusiastically welcomed their army's first foray into politics in the 1950s, they barely tolerate it now. Cohen stresses that this is an army that has lost its confidence and is searching for a new approach to Pakistan's fundamental problems.

The core of this new approach is Islam, and Cohen probes deeply into the program of Islamization that Zia has prescribed. Although there are many revealing insights, the discussion ultimately remains unsatisfying because the issue itself remains unresolved and ambiguous. Except for true fundamentalists, Islam can no more provide a guide to political behavior than any other religion. In an age of weapons of mass destruction and modern technology, it is a very questionable guide for an army, although there has emerged in Pakistan a doctrine for "Islamic" use of nuclear weapons that Cohen describes intriguingly and fairly. He also offers an intelligent analysis of the nuclear options open to both Pakistan and India—but it is one that will bring little comfort to those for whom nonproliferation is a sacred goal.

Cohen finished his book well before Zia's striking constitutional changes of 1985 and the return of Benazir Bhutto in 1986. These are events that have significantly altered the army's political role and it is still far from clear whether Zia and his civilian prime minister will be able to continue their attempts to introduce gradual change or whether the country will again be plunged into turmoil with yet another assertion of military rule.

Cohen's hopeful vision of a future that would relieve Pakistan of much of its strategic burden is a South Asia that achieves substantial autonomy from the inroads of the global system, but, as he points out, this can come about only in a context of supportive policies on the part of both the United States and India. This is perceptive analysis and wise advice, but it must be acted on by others and there is little to suggest that they are prepared to do so.

THOMAS PERRY THORNTON

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**Berlin between Two Worlds.** Edited by Ronald A. Francisco and Richard L. Merritt (Boulder, CO: Westview, 1986. xiii, 184 p. $16.50, paper).

No one ever claimed that the halving of Berlin in the aftermath of World War II was any action of Solomonic wisdom. Realpolitik and not biblical justice ruled the day. In this timely analysis of Berlin, East and West, 25 years after the construction of the infamous wall, we have an assessment of the consequences of those decisions of 1945 as well as some speculations about the future of the former capital of the Third Reich, one half of which is seen as an enclave of freedom, the other half as the capital of the dictatorial German Democratic Republic.

The book aims at a discussion of Berlin's future "from the perspective of all the major national actors involved." The essays issued from a conference in 1981 sponsored by the Aspen Institute Berlin; the International Institute for Comparative Social Research; and Science Center, Berlin with support from the West Berlin Senate. This volume concentrates on Berlin's external relations, while a companion volume, *Living with the Wall, 1961-1985*, edited by Anna and Richard Merritt (Durham, NC: Duke University Press, 1985), looks more specifically at the Quadripartite Agreement of 1971 and the implications of the Berlin Wall. The participants at this conference included policy makers, officials, and scholars with a strong contingent of U.S., British, and West German commentators.

The plan to locate Berlin between two worlds focuses on the East-West axis as the obvious polarity for this metropole, and the book approaches this theme through an excellent general introduction by Ronald Francisco in which the historical context, the major postwar developments, and the prospects for the future are clearly sketched. Francisco's essay also sets up the major questions that the other articles address more fully in headings that take first the Western Alliance and then the Warsaw Pact states as their topics, closing with two final essays on the future of Berlin.

The essays are well written and stimulating, but there is an unevenness to their quality. The piece by William Griffith, "The United States and Berlin" is long on getting a certain viewpoint out: that the Berlin relationship "arose out of a naive, dangerous American combination of unwarranted optimism about post-World War II Soviet policy toward the United States and the German Question." It is short on a careful scholarly analysis of the evolving U.S. position on the two Berlins. Since this is the only essay in the volume weighing the policy of the United States toward critical questions involving Ostpolitik and Deutschlandpolitik, we have Griffith's forceful and direct position about Berlin but not much on the reasons for those positions. In contrast to the other essays, this article is less scholarly and more journalistic.

A similar critique can be advanced for some of the other essays. Notable exceptions are the articles by Sodaro ("Berlin and the GDR: From Crisis to Bargaining Chip"), Francisco ("West Berlin and the Two Germanies: The Interplay of Political and Economic Motives"), and Wettig ("The Relations between West Berlin and the Warsaw Pack States"). These essays provide thoughtful analytic frameworks that hold together a wide array of facts with thorough scholarly documentation. Both Sodaro and Francisco use functional approaches and linkage analysis to assert a general thesis of the volume: Berlin is no longer the flashpoint focus of East-West relationships, and the adaptations of the two halves of this city are intricately enmeshed in East-West, East and West German, and East and West Berlin connections. The intense political, economic, and diplomatic bargaining (almost a baroque minuet) at the outer edges of alliance and satellite arrangements are perceptively presented in the Sodaro and Francisco essays. Wettig, an Eastern European specialist at Cologne University, examines the "logic of modus vivendi," the rhythms of detente and seclusion, and the politics brewed by two cities whose future destiny lies beyond the political sovereignties to which they are currently attached. West Berlin may somehow seem very much a part of West Germany as East Berlin claims to be the capital of East Germany, but the victors of the hot war that preceded the cold war really have the final say.

In addition to these essays, special note should be mentioned of Richard Merritt's article, "Postwar Berlin: Divided City." Using communications analysis, Merritt helps to move us from high diplomacy to ground level, where the changing political cultures of these two cities manifest themselves in institutions, buildings, streets, municipal services, and personal contacts. The obvious breakdown of the Berlin community into two separate subcultures is clearly documented. Merritt's work in this volume and in the companion volume continue his tradition as unofficial historiographer of Berlin.

One last comment about unevenness should be noted in a volume with considerable diversity of viewpoint. It may be a function of penetrating closed societies, but the speculation and analysis of the East German and Soviet political elites as far as Berlin is concerned is not very substantial. We know that Ulbricht fought Moscow over the Quadripartite Agreement of 1971, but our understanding of Honecker or of the various Soviet premiers is scarcely extended in some of these essays. If the keys to the future of the German question lie in Moscow and Potsdam, then we need more fleshed-out speculation on these elites. Berlin is between two worlds, and the scenarios for its future from the eastern perspective need more substance.

DONALD SCHOONMAKER

*Wake Forest University*

**Soviet Planning in Peace and War, 1938-1945.** By Mark Harrison (Cambridge: Cambridge University Press, 1985. xiv, 315 p. $44.50).

**Technocratic Socialism: The Soviet Union in the Advanced Industrial Era.** By Erik P. Hoffmann and Robbin F. Laird (Durham, NC: Duke University Press, 1985. ix, 228 p. $37.50, cloth; $12.50, paper).

**The Myth of the Plan.** By Peter Rutland (LaSalle, IL: Open Court, 1985. 286 p. $26.95).

At a time when the Kremlin has ushered in a relatively fresh new leadership and when reform has become a more realistic prospect, it is instructive to reconsider the theoretical and political underpinnings of the existing soviet system. Of particular interest are the old questions of why the system has turned out the way it has, what evidence there is of its capacity for change, what theoretic basis exists for change in the future, and which obstacles to change are likely to emerge and/or reemerge. These three books may not provide all the answers, but collectively, they make a good place to start.

Rutland's analysis of the theory and practice of planning takes the novel approach of using the Soviet Union as a case study in applied planning, rather than as the specific subject of inquiry. Hence, the analysis focuses on those elements of planning that would be found in any centrally planned system, Soviet-style or not, and that would, according to Rutland, ultimately disable the system. He argues this by applying Mises's and Hayek's writings on the organizational and economic impracticability of planning to the Soviet case, while taking some care to distance himself from the conservatism particularly implicit in Hayek. Indeed, he remains sympathetic to socialism as an idea; it is planning as its instrument that he questions and calls instead for greater reliance on the market under socialism as the more practical, if scarcely perfect, mechanism.

His other main point is that the plan is, in any case, a political tool rather than an economic one: "In the Soviet case, it became a vehicle for furthering political authoritarianism and the repression of individual rights. In certain developing countries it seems to have become a channel for political opportunism, corruption and patronage. In liberal democracies . . . it may become a battleground of the sort of rival claims on resources which dominate the democratic political arena" (p. 261). There is also a chapter on social-welfare planning, which perhaps overstates some conditions (such as infant mortality and the health risks inherent in female work force participation) but is nonetheless persuasive in showing that this is even more complex and less susceptible to rationality than economic planning.

The book's outstanding feature is both a strength and weakness. By arguing that the USSR is not a "special case," Rutland isolates the plan from its political and cultural environment and analyzes its functioning as a neutral instrument that just happens to be applied in the USSR. This does shed some useful light, but by the same token, as he himself argues, the plan is not in fact neutral but is explicitly political, and some of those features of the Soviet planning process and apparatus that he takes to be characteristic of plans may be characteristic of the Soviet environment instead. Finally, it is difficult to say who the proper audience is for the book. Little of it is really new. It is a synthesis of largely familiar Western writing, with little reliance on Soviet sources. Yet clearly it was not written for the layman, for if some sections are absorbing, others are quite heavy going. What saves the book are the odd but interesting insights scattered throughout it, such as a discussion of the ritual aspects of planning that enhance compliance while also increasing conservatism (p. 254).

The Harrison book cannot offer such erratic flashes of insight, but it is a meticulous piece of scholarship, with extensive research and dense detailing. It recounts the Soviet economy's transition to a war footing and back again, emphasizing the fundamental tension between the drive for mobilization, characterized by authoritarian management, and the need for economic balance, which involves rationalization and decentralization. Since this tension was present in the war economy as well as in peacetime, Harrison concludes that it is integral to the dynamics of Soviet development, with the war experience only reinforcing its impact. Thus the war taught the limits of management by decree, which was effective for mobilizing the war economy but not for sustaining it, while the retreat to balance after 1942 provided lessons for post-Stalin reformers. The implications of this for contemporary reformers is that past planning experience is the source of continuing authoritarianism and militarization of the economy, but it is also precedent for flexibility. The rise and fall of Voznesenskii, the advocate of balance, runs through the book as a sort of leitmotif, however, and is perhaps intended as an object lesson to those who would put economic considerations over political ones. In this Harrison and Rutland are agreed, that the plan is essentially a political instrument.

Hoffmann and Laird's book completes their trilogy on the impact of the "scientific-technological revolution" (NTR, from its Russian name), and joins *The Politics of Economic Modernization in the Soviet Union* (Ithaca: Cornell University Press, 1982). It begins with an exhaustive review of the now enormous Soviet literature on the NTR, that, like most such reviews, concludes that the term remains undefined. While this one is clear and up-to-date, it is probably unnecessary to provide such a huge sample of current Soviet thinking on the subject. After two and a half chapters describing Soviet theorizing on the NTR, "developed socialism," and the "scientific management of society," the authors get to the heart of the matter: the impact of all these theories of the influence of technology on actual policy, which, they conclude, is not much. In discussing such issues as the role of experts, computers, and the mass media, they illustrate well the conflicting demands of the NTR (greater openness, decentralization, and rationalization) and of the status quo, which resists such developments out of inertia and out of protectiveness for vested interests. In this regard, Hoffmann and Laird complement Harrison's historical analysis, while their conclusion makes common cause with both of the other books reviewed here. Politics, rather than the demands of economic or technological development, drives policy making: "Politicized orientations and choices, not the inexorable demands of . . . scientific-technological and socioeconomic forces, have decisively influenced . . . politics. Technological spontaneity has shaped but not mastered political 'consciousness'" (p. 200).

One is tempted, while slogging through NTR theory, to dismiss it as much ado about nothing—as either empty and sterile, with no impact on real-world policy making, or as post facto justifications for the leadership's essentially atheoretic decisions. The authors' contribution here is to show that there is indeed some substance to all this, even if it has been difficult actually to implement NTR-inspired change. It remains to be seen, whether the "reciprocal influences" they trace between the NTR and politics will generate "technocratic socialism," a new variety of technologically enhanced authoritarianism, albeit with increased mass participation at its fringes. Even if its seven features do develop, one wonders whether it really constitutes something new, as opposed to merely something more efficient. A final note: this book has a truly terrible, names-only index; it deserves better.

None of these books breaks new ground, yet all contribute something to the ongoing debate on the prospects for reform. And all in their own ways offer mixed reviews of this. Rutland is most pessimistic (and most provocative), but also least apocalyptic, in that the system seems to him able to "soldier on" without benefit of substantial change. The other two, however, remind us that while the soviet system has never adapted itself to new conditions *easily*, it *has* adapted and thus set ample precedent for change in the future.

ANDREA STEVENSON SANJIAN

*Bucknell University*

**Higher Education and the State in Latin America: Private Challenges to Public Dominance.** By Daniel Levy (Chicago: University of Chicago Press, 1986. xvii, 434 p. $27.50).

This is an excellent study, a "contribution to the literature" that will most benefit specialists in comparative-education administration or policy studies. It will be somewhat less useful to students of comparative politics or to Latin Americanists in general. Let me explain why before turning to the positive substance of the book.

Generally, one should not judge a work in terms of what the reviewer would like to do if he/she ever gets around to doing it rather than what the author sought to do. In this case, however, Levy suggests in his very first chapter, "Issues and Concepts," that he will cover the politics of certain aspects of higher education in Latin America. Instead, his primary emphasis is on the role of private institutions, both religious and secular, in higher education and their relations with public institutions and the state. This is a perfectly valid topic for investigation, but it focuses attention on only one rather specific aspect of a broader set of political concerns, especially as Levy seems to regard politics in terms of his principal specialties of educational administration and policy studies. Lasswell's classic definition of politics, "who gets what, when, how?" gets rather short shrift in public-policy studies of this sort, probably because the theoretical framework raises the nature of consideration to such a high level of abstraction that *who* becomes a modal set of disembodied interests, *what* a series of idealized societal goals, and *how* a kind of bloodless exercise in game theory. And Lasswell's implicit question, *why*, seems bogged down in the morass of organizational theory.

This is not to suggest a lack of valuable material in the theoretical portions of the study as well as in the general background and conclusion sections of the work. The case studies representing typological differences in public-private university relationships in Chile, Mexico, and Brazil, as well as among the remaining Latin American university systems, include rich substantive content. But the facts, and even their interpretations, are in the main incidental to the logical and theoretical thrust of the presentation.

For example, the struggle between religion and the secular state is discussed more as a facet of the competition between state-dominated "public" and church-dominated "private" institutions than as a broader battle for social, economic, and political power in the general post-independence environment. This is a perfectly valid emphasis within the context of Levy's study, but one that offers a very unbalanced view of the problem for the generalist. A second example: in the book, basic organizational patterns that determine the role of the Latin American university either are assumed to be known to the reader or are buried in the end notes. The structuring of university operations through *facultades* rather than departments, which clearly affects both the nature of higher education and its appropriateness to a rapidly shifting environment, is not fully or directly addressed. Thus, pressing political problems are played down in favor of questions of organization theory or public-policy concerns. Again, this emphasis is valid in its own right, but more specialized than the generalist would prefer.

In sum, this excellent and penetrating study has great value for the theoretically oriented specialist but less value for the generalist.

ROBERT E. SCOTT

*University of Illinois, Urbana-Champaign*

**Women and European Politics: Contemporary Feminism and Public Policy.** By Joni Lovenduski (Amherst: University of Massachusetts Press, 1986. xiii, 320 p. $27.50, cloth; $10.95, paper).

The comparative approach brings intriguing perspectives to the study of women and politics, as Lovenduski's work illustrates. Her book analyzes the marked variations in women's political influence across Western and Eastern Europe. As she points out, Europe is a superlative object of such a study. Although the nations have certain shared historical experiences and similar economic-development levels, they also differ enormously in cultural norms about women's political role and in the institutional and political frameworks in which women (and men) operate.

Lovenduski first analyzes the growth of the feminist movement in the nineteenth and early twentieth centuries (the "first wave") and since the mid 1960s (the "second wave"). The strength of these movements varied from nation to nation, and these variations further differed across the two waves. In both, the author finds the political setting to be a major cause of the cross-national variations. For example, in the first-wave countries more moderate political traditions, such as Britain and Scandinavia, had more influential feminist movements that were not deeply split between socialist and bourgeois wings. Moreover, the second wave of the feminist movement was most successful when allied with both left-wing groups and traditional women's organizations, while concurrently avoiding preemption by political forces outside the movement. Thus, for instance, the women's liberation movement has been more powerful in Britain, Italy, Norway, and Denmark than in West Germany, France, and Sweden.

Lovenduski then assesses women's current political status with a variety of indicators. She presents extensive data on women's representation among the political elite, in legislative, executive, senior civil-service, and judicial positions. She also reviews the literature on sex differences in mass-level types of political participation. Most unusual, she gives detailed descriptions of women's position in political parties and trade unions. Until now, this information has been scanty, and this was especially unfortunate because these organizations are usually quite influential in European politics. Political parties are the major gateway to elite political positions, while economic interest groups, including trade unions, are often directly consulted in policy development. The women's liberation movement sparked demands for better representation of women and women's concerns by these organizations. Lovenduski describes how these demands met with more or less success, depending on the organization and country.

Lovenduski concludes with a discussion of the policy successes and failures of second-wave feminism. She focuses on two types of policies: (1) women's employment opportunities and conditions and (2) abortion and reproductive rights. She shows how policy outcomes do not simply reflect either the strength and activities of women's groups or women's general political power. An additional important influence on these and earlier policies is "state imperatives of economic growth, population replacement, and political stability" (p. 295).

Two other books discuss similar topics in a comparative perspective. Vicky Randall's *Women and Politics* (London: Macmillan, 1982) and Joni Lovenduski and Jill Hill's edited work, *The Politics of the Second Electorate* (Boston: Methuen, 1981). In fact, the acknowledgements indicate mutual consultations.

Lovenduski's new book differs in several important ways, however. First, of course, it has more current material. Also, it confines itself to Europe, which allows for deeper analysis. Information is still quite limited for Eastern Europe, however, particularly in comparison to that available from Great Britain, France, West Germany, and Italy. Finally, the book welds concepts and approaches from comparative politics onto the concerns identified by feminist theory. The comparative perspective is most fruitful for explaining why the feminist movement has been more influential in some countries than others. It turns out that the results are quite complex; nations that rank high by one indicator may fall low by another. Lovenduski's explanations for these results show great sensitivity to political and institutional factors. However, perhaps unavoidably, the book is less original in its analysis of broader issues, such as why women have so little power in the first place and what should be done to empower women.

CAROL A. CHRISTY

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**Urban Social Movements: The City after Castells.** By Stuart Lowe (New York: St. Martin's, 1986. 211 p. $27.50).

Over the past decade or so, the subjects of urban sociology and urban politics have undergone marked changes in their tone and content. Perhaps the chief initiator of these changes has been Manuel Castells, whose work has focused on the role of nonparty urban social movements as generators of social change. Now, a decade after his seminal work, *The Urban Question*, first appeared in English, it is possible to put into perspective not only the nature of his views but also the ways in which they have changed over time.

Stuart Lowe's book allows us to trace the development of Castells's views. He summarizes Castells's conception of urban social movements as "organisations standing outside the formal party system which bring people together to defend or challenge the provision of urban public services and to protect the local environment." He then identifies three phases in Castells's treatment of such movements. Initially Castells argued that urban social movements must align themselves behind the vanguard of the working-class movement in order to effect social change. Subsequently he moved away from such a class-oriented position, identifying new forms of social cleavage arising from public and private sector consumption patterns. This led him to see urban social movements as potential elements of an interclass progressive political alliance. Finally Castells has come to see urban social movements as the only genuine sources of social change, concerned with the transformation of values and meaning systems. Political parties are now relegated to mere bargaining devices. In Lowe's words, Castells "has swung from support for the vanguard organisations of the workers' movement, to the Eurocommunist popular front, and now writing from the USA, has embraced non-class 'alternative' and counter-cultural movements."

In addition to tracing the evolution of Castells's ideas, Lowe provides case studies of urban social movements in Britain, Europe and the Americas and reviews Castells's attempt to construct a cross-cultural model of urban social change that might encompass them. He sees the attempt as unsuccessful, not least because of its reliance on the case of the Madrid Citizens movement whose emergence at the end of the Franco era surely marked it as the product of certain unique historical circumstances rather than as an example of some generic model.

Finally, Lowe suggests four analytical themes that might assist our understanding of urban social movements. These relate to (1) the process of mobilization and concern the nature of the social base (e.g., class, locality, consumption stakes); (2) the nature of local political power, including the role of parties; (3) the particular ideologies that pervade the urban system; and (4) the general economic climate.

This is a useful and stimulating book that deserves, and will doubtless find, a ready audience amongst students of urban affairs. For a British reviewer, two particular points give food for thought. One concerns his characterization of the "local socialism" of Britain's urban Left in some of the major cities as being akin to Castells's "broad left" middle-phase position. Another is his identification of "the strong tendency . . . towards long-term, one-party political control of local government" as a major determinant of the character of urban social movements in Britain. Local election results in 1985 and 1986 have modified this tendency. About one third of England's population now live in areas where the major local authority is "hung," under a three-party system, with no single party in majority control. It remains to be seen what the consequences of this development will be for urban social movements in those areas.

JOHN GYFORD

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**Venezuela: The Democratic Experience.** Edited by John D. Martz and David J. Myers (New York: Praeger, 1986. xxiv, 485 p. $33.95).

This is an updated version of a collection first published in 1977. Since the first edition appeared, Venezuelans have further consolidated and strengthened their two-party system. They have also passed through a remarkable boom and bust cycle that has left the nation facing severe economic difficulties, with reduced income and high debt. At the same time, the recent return to civilian rule elsewhere in Latin America (played out for the most part in similarly difficult economic circumstances) has seen central aspects of Venezuelan political experience repeated time and again, especially the characteristic stress on social pacts and interparty accommodation. For all these reasons, this is a particularly good moment to take a fresh look at Venezuela's experience with democracy, and to reassess its broader implications.

A number of chapters from the first edition have been revised and updated. There are also new authors and new contributions, for example, in chapters on state enterprises, education, and agriculture. The volume is divided into three parts, along a logic loosely derived from Almond and Powell's *Comparative Politics* (Boston: Little, Brown, 1978) Part 1 ("Environments") contains papers on history, constitutional forms and realities, recent trends in public opinion about democracy, and the politics of economic development. Part 2 ("Articulating and Aggregating Interests") has chapters on the party system, the political role of the military, state enterprises and decentralized public administration, and public administration as a whole. Part 3 ("Policy and Performance") reviews the record of policy and policy making in petroleum, education, agriculture, Indian affairs, local government, and foreign relations. The editors close with a general summary of performance and a brief sketch of prospects for the future.

It is a cliche of book reviewing to note the unevenness of edited collections. But like all cliches, this one has a core of truth that is well illustrated in this case. Contributions to this volume vary enormously in theoretical focus and concepts, as well as in the richness, complexity, and scope of data analysis. The best chapters (Baloyra on public opinion, Ellner on educational policy, Bigler and Viloria on state enterprises) provide substantial new information. The most disappointing chapters (Kelly on constitutional form and reality, Gil Yepes on the military, Stewart on public administration) remain at very general levels of analysis, while advancing confused and often contradictory concepts and explanations.

The volume would have benefited from tighter editing. As it stands, there is no common approach and no shared set of basic concepts. Contributors use very different terms to describe the contemporary Venezuelan political system. Some apparently regard it as a kind of liberal pluralism. But Gil Yepes borrows diametrically opposed concepts of "organic statism" to build a thesis about the role of the military. Finally, a number of authors use Veliz's ideas about "Iberian centralism" in a loose way to explain everything about Venezuelan policy and politics, under democracy and military rule alike.

Tighter editing would also have ensured more consistent empirical coverage. I have already pointed to differences in the complexity and detail of data analysis. There is also considerable variation in historical scope across the chapters. Some focus on the last decade, others review the whole democratic period, and a handful reach back into colonial times for models and illustrations. Further, there seems to be no clear criterion for case selection. Why have a chapter on Indian affairs and nothing on organized labor? Why include a chapter on agriculture without one on industry? Given the editors' stated goal of providing a "broad overall reevaluation of Venezuelan democracy" (p. vii), general overviews of the development of social structure, electoral behavior, and the economy, or of issues like generational transfer would have been helpful, as would a fuller and more consistent treatment of statistics, perhaps in an appendix.

These considerations point up the absence of a unifying focus: there is no general introduction, no synthesizing conclusion, and no overarching perspective to tie the chapters together. It is therefore difficult for the editors to draw comparative references. This is too bad, for as I noted earlier, the study of Venezuela has much to reveal about the nature and possibilities of change elsewhere in the region. Possible comparative insights into the costs and benefits of alternative political, economic, and fiscal strategies are not exploited, and thus the opportunity for more general analysis of the bases of democratic consolidation is lost. This suggests that while some chapters will be useful to specialists, as a whole the volume unfortunately is of more limited interest.

DANIEL H. LEVINE

*University of Michigan*

**Conflict and Compromise in Multilingual Societies. Vol. 2, Belgium.** By Kenneth D. McRae (Waterloo, Ontario: Wilfrid Laurier University Press, 1986. xiv, 387 p. $28.95, cloth; $12.50, paper).

Belgium's language dichotomy is one of its most obvious characteristics and its most intricate sociocultural and political problem. Conflict between Flemings and Francophones has been a salient feature of Belgian politics for generations. Yet for all its intractability, the Belgian "communal problem" has not resulted in much bloodshed, even compared to other European ethnic conflicts such as Northern Ireland or the Basque situation. How has Belgium escaped the violence that so often accompanies ethnic hostilities? Kenneth D. McRae's analysis goes far toward providing an answer. His book is the most comprehensive study of the impact of language conflict on Belgian society and politics that we are likely to have for many years. A worthy successor to classic English-language studies of Belgium's communal disputes by Shepherd B. Clough and Val R. Lorwin, it should become the indispensable starting point to every serious investigation of Belgian-language politics and policy in the future.

Drawing on a vast array of source material in four languages and on discussions with well-informed Belgian and foreign observers, McRae integrates sociolinguistic and sociological perspectives with the more familiar historical and political approaches. After an excellent historical overview, he presents well-selected data on the geographic and sociological patterns of language use, group images and stereotypes, divergent values, and the interaction of language with other cleavages and conflicts. In addition, he offers nearly a hundred pages of valuable description of the evolution of language regulations and practices in government, the public service, the courts, the armed forces, education, and the private sector.

Of particular interest to political scientists is McRae's treatment of the politics of constitutional amendments and special concurrent majority laws designed to introduce cultural autonomy and regional decentralization into the traditionally unitary Belgian state. Some powers previously reserved to the central parliament are now exercized by subnational legislative bodies, the Flemish and Francophone Community Councils and the Walloon Regional Council. But the independent taxing powers of the new councils remains very circumscribed, their membership is composed of legislators from the central parliament doing double duty as regional lawmakers, and the problem of how to fit Brussels into the new scheme remains unresolved. Thus, while the post-1980 Belgian system can be classified as "a unique combination of territorial and non-territorial federalism," it is also "a weak and cautious federalism . . . still entrammelled by a century and a half of unitary habits of mind" (p. 171).

This book is the second of four national case studies McRae is planning. Volume 1, *Switzerland,* was published in 1983 by the Wilfrid Laurier University Press, and studies of Finland and Canada will follow. These nations are developed democracies that have handled linguistic pluralism with a minimum of overt violence. McRae hopes to use his intensive studies of these "successful" cases to assess some general hypotheses about linguistic diversity and plural societies. At the midpoint in his efforts, McRae's forte is clearly empirical description and comparative analysis. He points out that Switzerland largely avoided politicizing linguistic differences, thanks to her decentralized federal system, the relative international equality of her major languages, and the lack of reinforcement between language and other social cleavages. In Belgium, language conflict was politicized by the unitary system to the point that it became all-pervasive and required major structural adjustments to key political institutions.

Belgium's detailed language laws, delicately balanced constitutional amendments, and the tortuous process of negotiations that produced them undoubtedly play an important role in undercutting popular support for extremism and violence. People cannot work up a fever pitch of ethnic nationalism over the Permanent Linguistic Control Commission's latest ruling on the required percentage of Dutch-speaking employees at the National Savings and Loan Association. Thus, at a certain cost in economic efficiency, the Belgian political elite has managed to formulate linguistic questions in ways that make them seem not worth dying—or killing—for. Yet, the new institutions keep linguistic questions always on the public agenda. Routinized linguistic squabbling thereby becomes an officially sanctioned national vice, like a state lottery in a country of compulsive gamblers. No wonder, then, that McRae concludes "paradoxically . . . Belgium seems fated to remain for some time to come a polity integrated by the very intensity of distrust across its regional and linguistic boundaries" (p. 171).

JAMES A. DUNN, JR.

*Rutgers University*

**State and Peasant Politics in Sri Lanka.** By Mick Moore (New York: Cambridge University Press, 1985. xv, 328 p. $44.50).

The peasantry in most Asian and African societies has little influence over policy making and implementation. What is surprising is the extent to which this phenomenon *is* found in many countries. Mick Moore examines peasant politics in Sri Lanka, a society where one would expect more influence on the part of the peasantry. Sri Lanka is known for its adherence to representative democracy and its provision of social welfare to its people. In addition, in the 1950s, a populist party gained power emphasizing the rural traditions of the society. However, despite these accomplishments, the Sri Lankan peasantry does not have extensive influence over policy making.

Moore's book addresses this failure of the agrarian class to influence the government effectively. The study begins with a good, thorough description of agricultural policies in a series of chapters discussing crown lands, land tenure, commodity pricing, and agricultural services. In chapter 6, he discusses the theoretical framework that will structure the remainder of the book. Using a neo-Marxist approach that is devoid of ideological rhetoric, he presents a core-periphery framework from which to examine the final questions addressed in the book. Chapter 7 examines the rural smallholders as an identifiable interest group in the society. This is followed by a description of the rural identity and a cultural explanation of the political impotence of the smallholders. Chapter 9 argues that the impotence is due partially to the ethnic crisis between the island's two main ethnic groups, the Sinhalese and the Tamils. Chapter 10 presents an institutional explanation for the powerlessness, arguing that the nation's elite and political structure have helped limit the peasants' political influence.

Moore is an informed and insightful student of Sri Lankan society and politics. His analysis is strengthened by viewing the problem from the peasant viewpoint. Much of the literature on Sri Lankan society proceeds from the top down, examining the polity from the upper levels of government. The author's examination of the behavior and attitudes of the peasant population through participant observation is, however, highly impressionistic. At times, some of his premises and conclusions are not convincing. Although the conclusions are often plausible, they frequently are based on limited or questionable data. A number of conclusions are based on combining and analyzing data spanning almost 20 years with little concern about the possible time dependency of the data. At other times the data are not there. For instance, he argues that class-based voting is common in Sri Lanka without presenting adequate evidence to support the argument. The reader is left to accept the author's conclusion blindly, as it becomes a premise for later arguments.

The major weakness of the book is its lack of substantiation. Moore makes sweeping generalizations, and while many of the conclusions appear to be plausible, the premises they are based on appear questionable. The strength of the book lies in the application of an interesting theoretical approach to the topic. Moore's neo-Marxist approach places a great deal of emphasis on the impact of the state on the behavior of the rural smallholders. He does not restrict the focus of his analysis to a simplistic class-based analysis.

Despite its shortcomings, the book is an important contribution to the literature on peasant and South Asian studies and should be read by all scholars interested in these topics. Moore offers scholars of peasant politics a model worth noting and testing in other situations and offers other scholars new insight into the nature of rural politics in Sri Lanka and developing societies.

ROBERT C. OBERST

*Nebraska Wesleyan University*

**The Triumph of Democracy in Spain.** By Paul Preston (London: Methuen, 1986. xi, 274 p. $32.00).

**The Making of Spanish Democracy.** By Donald Share (New York: Praeger, 1986. xvii, 230 p. $38.95).

If Preston's study were more dispassionate, more reasonably focused, and less shrill, it would make an interesting companion piece to Share's study, which presents a disinterested, careful exposition and analysis of the transition from dictatorship to democracy in Spain following the death of Franco. Share centers his attention, as he should, on the extraordinary accomplishments of Adolfo Suarez, prime minister from July 1976 to January 1981 and King Juan Carlos, yet never minimizes the threats from the Ultras of the Franco regime and from the regional Basque terrorists, the Euzkadi ta Azkataruma (ETA [Basque Nation and Liberty]), both of which set out to destroy the embryonic democracy. Share keeps in perspective the conspiracies and the violence—which ultimately failed to achieve their purposes—and never loses sight of the long-range democratic goal that was achieved.

Preston, by contrast, centers his attention (his passion, really) on the conspirators and the perpetrators of violence, leading the reader almost to believe that the undemocratic forces were stronger than the democratic ones and that democracy triumphed in spite of itself. Preston simply cannot forgive Suarez for having emerged from the Franco regime, and only rarely does he mention the prime minister's name without adjectives that mock his pragmatism and his manipulative skills, which—while not necessarily immaculate—sought to offer something to almost every group in order to insinuate as many as possible into the consensus that brought about the miracle of the transition to democracy. Preston is the leftist romantic who judges all such accommodative behavior to be impure and who really would have preferred to see Spanish democracy emerge from a clean break with the past, a *ruptura*, as the Spaniards call it. Preston's preferences, which he might believe do not show, contort his evaluation of the transition. His heart runs away with his head, and he exhausts the thesaurus (and the reader) with words that act like leitmotifs, trumpeting contempt for those he sees as the villains and approbation for those he accepts as the heroes—just in the event that the readers might choose to think for themselves and draw their own conclusions.

Preston's most serious shortcoming, of his own making, is the failure to take his readers where, early in the monograph, he implies he will take them: into the smoke-filled rooms where, away from public scrutiny, the deals were cut by Suarez and his colleagues with both the Right and the Left opposition. Preston whets the readers' curiosity, for as yet no outsiders (read: scholars interested in the transition) have been in those rooms. But Preston takes us as far as the door and no further, along paths whose outlines are already familiar to careful students of Spanish politics. There is an almost total and glaring omission of even an attempt to analyze the labyrinthine maneuvers that produced the constitution, the centerpiece of the new political order. Nor does Preston take the readers into the rooms with the Ultras or the ETA, not an unreasonable expectation given the focus of the book. Thus, his study does not measurably further our knowledge of either the mainstream of Spanish politics from 1975 and 1982 (when the book ends) or of the undercurrents of conspiracy and violence, which still, for the most part, lie hidden to research.

Share does not take us inside the rooms either. He never claims or implies that he will. But he examines the largely familiar paths to them with such clarity, freshness, and detail that the reader is swept into the mainstream of Spanish transitional politics by his vigor and rigor. Share accepts without lamentation the reality that the transition was the creation of political elites from the moderate Right to the moderate Left who, of necessity, operated largely out of view of an expectant, but politically unsophisticated and unpracticed, public. Share understands with the mind of a non-ideologue that the only transition to democracy that was possible without risk of provoking the military would be one led by moderates from the Right like Suarez with ties to Francoism who, while acting within the laws of the Franco regime, could both ally with the moderates from the Left (and even some not so moderate like the Socialists and Communists before the two parties publicly shed their more extreme identity) and finesse all but the Ultras on the Right. The diehards would be unable to stop the transition without the support of the majority of the military elite, who were carefully nurtured, but kept in tether, by Juan Carlos. The king's extraordinary efforts paid off in February 1981, when a coup d'etat was put down before it could spread. The "transition through transaction" had survived its baptism by fire.

Surely no one is so naive as not to believe that the moderate Right got more out of the transition than the moderate Left and that many who profited under Franco still profit today. But profound socioeconomic change was simply not a viable option in the years following Franco's death. Those who, like Preston, sincerely and deeply feel that a great opportunity to restructure Spain was missed can take hope and pride in the fact that after the debacle of the Union de Centro Democratico (UCD [Union of the Democratic Center]) the Socialists came into power in Spain in 1982 under the brilliant leadership of Felipe Gonzalez and still govern as this review is written, a phenomenon that in itself is revolutionary for Spain.

E. RAMÓN ARANGO

*Louisiana State University*

**Japanese Prefectures and Policymaking.** By Steven R. Reed (Pittsburgh: University of Pittsburgh Press, 1986. ix, 197 p. $26.95).

This study evaluates the distribution of influence in Japanese intergovernmental relations through detailed case studies of three policy areas (pollution control, housing, and high-school education) in three of Japan's forty-seven prefectures. Reed presents six hypotheses drawn from the comparative literature in this field. His major findings are that neither legal nor financial authority is necessarily related to influence; that control over information does, on the other hand, make a difference; that the substantive merits of a case can provide effective leverage in intergovernmental negotiating; that the least-interested party in a venture often exerts the greatest influence; but that, in the end, the single most important factor accounting for influence is politics.

Having taken the analysis this far, in a succinct and closely argued manner, Reed then makes the astounding statement that he "cannot give Japan high marks for local democracy" (p. 168). The basis for this conclusion is the perceived weakness of partisanship at the local level, the relative passivity of local assemblies, the tendency toward noncompetitive elections, and the lack of ideological fervor in debating local issues. Does this then mean that local residents are not free to choose their leaders, that their interests are not adequately represented in local administration, or that elected officials cannot establish local priorities reflecting the will of the people? By these measures, the evidence of above-average performance and responsiveness on the part of local authorities, even when compared to their counterparts in such democratic bastions as England or the United States, suggests otherwise. And this, in turn, points to the need for other, more contextually derived, indicators of democracy and local autonomy than those normally found in the literature on the Western democracies.

In the end, Reed's somewhat overdrawn and weakly substantiated hypotheses serve to underscore the depressing hollowness of much cross-national analysis. To simply say that "Japan is more centralized than the United States or Canada, but less centralized than France" (p. 163) leaves us groping in the dark, wondering what we have really learned. The hazards of postulating functional equivalence without adequately accounting for major systemic differences are glaringly revealed in this approach. Better to describe in richer detail what prefectural politics is actually all about, how it has developed since the U.S. Occupation, how it frequently collides with the interests of constituent municipalities and the ever present central government, and its future in the larger scheme of things.

Prefectures have become important enough in national political life to stimulate talk of their abolition and replacement with less effective regional governments. But the voters and their politicians feel otherwise and want prefectures to exist, both for the political leverage they provide in national policy making and for their demonstrated effectiveness in parceling out the fruits of economic success. A healthy democratic sense of fair play keeps the prefectures in business and has made them highly resilient. If our normative categories are not adequate to describe this situation, then we should create new categories.

RONALD AQUA

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**Democracy at Work.** By Tom Schuller (New York: Oxford University Press, 1985. 190 p. $19.95).

There was a time almost 20 years ago when industrial democracy was a fashionable and much discussed idea. Political scientists wrote enthusiastically about the value of participation, and they wrote critically of the apathy that they identified as an integral part of pluralist accounts of democracy. Since then, political interest in industrial democracy has waned, particularly in the UK, and the books now written on the subject have adopted a more cautious, less utopian, tone. Tom Schuller's book fits into this latter tradition.

*Democracy at Work* reassesses the claim for industrial democracy in the light of the evidence of the early experiments in participation and in the light of changes in the nature of industrial organization. The book is both a study in political argument and a sociology of industry. It seeks to relate the abstract concepts of democracy to the practical realities of industrial life. Schuller makes no secret of his belief in the need for a "more equitable distribution of power at the workplace" (p. 3); his intention, though, is not to write a polemic but instead "to provide a relatively straightforward way of looking at the general theme of worker participation" (p. 3). This is precisely what he does.

There are a number of features of Schuller's argument that are particularly worth noting. First, unlike some other writers in this field, he recognizes that the object of democratization, work itself, is not a simple entity. The shift from manual to clerical employment, the fragmentation of union organization, and the growth of part-time work all pose new problems for increasing participation. Secondly, Schuller emphasizes that participation, too, comes in a variety of forms—from collective bargaining to having workers on the board to forms of worker ownership. Depending on how and where they are applied, different forms will have a different impact on the balance of industrial power. Thirdly, he argues that industry is not neatly divided into two sides and that interests do not fit into discrete categories. Such points are part of Schuller's constant theme: conflict is an inevitable feature of industrial life. Democracy will not eliminate tension and issues cannot be separated into those that are inimicably divisive (wages) and those where a common interest predominates (health and safety).

These general arguments are related to Schuller's timely analysis of the changing character of the industrial enterprise. He emphasizes, for example, the increasing role played by pension funds. Not only do these provide a new source of industrial power, they also constitute a new element in the interests workers have in the outcome of industrial decisions. He considers, too, the role of technology in the division of labor and the consequences of deskilling on worker participation. He uses this discussion as an opportunity to make the valuable point that participation itself is a skill and one that needs to be learned by managers as well as workers.

Having provided so solid a foundation for the discussion of industrial democracy, Schuller rather frustratingly slides over some of the crucial issues he raises: the competition between efficiency and democracy and the tension between political and industrial representation. While he readily acknowledges that some sacrifice of efficiency may be entailed by an increase in participation, he says little about what tradeoff is appropriate. What price is worth paying for participation? It is noticeable, too, that he has little to say about the rights of consumers. The point of industry is, after all, to produce goods and services. Finally, political scientists may be disappointed that Schuller is concerned neither with the theoretical underpinning of the arguments for democracy nor with the political changes that accompany changes in industrial decision making.

Such complaints may, however, seem unreasonable criticism of this short (very readable) introductory text. *Democracy at Work* succeeds by stimulating thoughts on the subject of industrial participation and by setting a model for the combination of theoretical and empirical concerns. In an area once characterized by wishful thinking, Schuller has written a book where the thinking is refreshingly clearheaded.

JOHN STREET

*University of East Anglia*

**Power in the Caribbean Basin: A Comparative Study of Political Economy.** By Carl Stone. Inter-American Politics Series, vol. 5 (Philadelphia: Institute for the Study of Human Issues, 1986. xii, 159 p. $29.95).

A virtual tidal wave of books has accompanied the latest surge of revolutionary activity and national independence in the multicultural Caribbean Basin. But, as this reviewer noted in a recent coauthored essay covering nearly two hundred volumes *(Choice* [April 1985]), only a minority of authors investigating the region, albeit a steadily increasing number, demonstrate the inclination or skill to conduct truly cross-basin research. In this volume, Carl Stone, chair of the government department of the Jamaican campus of the University of the West Indies and probably the Caribbean's foremost survey analyst, makes a solid contribution to the cross-cultural approach.

Stone, whose *Democratic Clientelism in Jamaica* (New Brunswick, NJ: Transaction Books, 1980) remains the premier political assessment of the most populous Anglophone Caribbean state, presents an ambitious comparative political analysis of approximately two dozen political systems in the Caribbean Basin, excluding only Mexico, Colombia, and Venezuela. After discussing the impact of European colonialism on the basin, Stone groups the basin regimes typologically for purposes of comparison into the liberal democracies (Costa Rica, the Dominican Republic, and all Anglophone Caribbean states except Guyana and Grenada); the populist-statist regimes (Grenada, Guyana, Cuba, and Nicaragua); and the traditionally authoritarian states (the remaining Central American states plus Panama, Suriname, and Haiti). Other variables considered include economic openness and dependency, cultural modernization, militarization, economic ideology, and East-West identity. Correlations of regime type and the remaining variables to social, economic, and political outputs during the 1957-80 period make up the thrust of this volume.

Unlike the British, French, and Dutch colonizers of the basin, writes Stone, "Spain and Portugal failed to develop into centers of world capitalism or to evolve liberal democracies" (p. 3). Thus, in sharp contrast to the English-speaking Caribbean, where strong infusions of liberal democratic influences fostered widespread plural democracy, the Hispanic states of the Americas inherited a legacy of corporatist state management and military authoritarianism. While by no means an original thesis, this is an important assessment for impatient U.S. policy makers and observers, as is the generalization that institutionalized democratic pluralism serves as a major antidote to potential revolution.

Stone finds that between 1957 and 1980 reformist capitalist democratic systems as a group experienced a growth in per capita income of $762 as compared to only $170 in the authoritarian systems. The non-oil-exporting democracies were led by a $1,250 income expansion in Barbados, shifting its placement among basin states from eleventh to third, while Puerto Rico was declining from a preeminent position in 1957 to fourth place in 1980. Ironically, the Barbadian "bee-hive" model was heavily influenced, at least in its initial stages, by Puerto Rico's "Operation Boot Strap." Trinidad and Tobago, on the strength of petroleum exports, moved from third to first place with a $2,080 per capita income.

Costa Rica was the leader by a substantial margin in real wage increases during this quarter century, followed by the Bahamas, Barbados, Jamaica, and Trinidad, while other countries in the basin suffered declines! The countries with increases were all plural democracies *but* with a considerable range of private-sector, as opposed to state-controlled, systems. Countries from each end of the ideological scale, Cuba and Barbados, led the region in the increased quality of life and egalitarian income-shift categories. Trinidad and Costa Rica also received First World Physical Quality of Life Index (PQLI) ratings (1979) of 96 and 89, respectively. However, Stone concludes, it is important to look behind such statistical measurements. In the Cuban case, for example, twice as many Cubans as Barbadians or Costa Ricans die as a result of poor nutrition even though infant mortality rates in Cuba are lower. And Cuba has simply exchanged dependency on one superpower for dependency on another, while maintaining its monocrop dependence. Deep dependency problems are also endemic to Barbados. International profit taking exceeded investment during this period in Barbados (and to a much greater degree in other basin countries). This leads Stone to argue that development models focused on Confederation of British Industry-type (CBI) international investment are, at best, only a short-term solution.

A recurring theme in Stone's analysis is that generalizations must be examined with great care even when they are empirically based. Individual states in the basin with above-average income growth during the period of this study, for example, include Barbados, Trinidad, Cuba, Guatemala, Panama, and Costa Rica—*a most varied* mix of systems both in economic and regime orientation! "This pattern of results does not support [generalizations] which associate growth capability with particular ideological impulses, regime types, or political economies," Stone states. "Factors such as orientation of leaders, strength of institutions, control of population growth, innovative managers and entrepreneurs, and sound and effective management systems are more important [as determinants]" (p. 92). But even when regime type and economic ideology *are* important determinants, Stone appears to say that metropolitan policy makers are ill advised to force-feed models of governance or economics into unaccepting cultures or to make sweeping and overly romantic or denegrating assertions about specific models.

Errors are minimal, considering the magnitude of Stone's cross-basin research. There is a slight tendency to overstate, for instance, when Stone asserts that democratic pluralism permits "radical leftists to compete for power without resorting to violence" (p. 41). It is also difficult to accept Stone's generalization that Nicaraguan politicians possess "delicate coalition building skills" and "sensitivity for sustaining internal political unity" (pp. 147-48). Were this but the case! Most important, it is unfortunate that data were not available to carry this study forward into the 1980s. Finally, in this reviewer's judgment, the external variable is considerably understated as an independent variable in the development of the Caribbean Basin.

But these criticisms are mere specks on a solid, empirically based, cross-basin analysis that is all the more important in view of the current emphasis on geopolitics in the Caribbean Basin. This volume will probably be much emulated in future comparative works. Highly recommended.

W. MARVIN WILL

*University of Tulsa*

**The Cultural Revolution and Post-Mao Reforms: A Historical Perspective.** By Tang Tsou (Chicago: University of Chicago Press, 1986. xiv, 351 p. $29.95).

This book deals with the tortuous course of China's political development in the twentieth century, viewing the rise of the Chinese Communist state as an adaptive response to the "total crisis" in which Chinese society found itself in the early 1900s. Nonetheless, some of the very features of the ideology and organization that enabled the Communists to defeat the Guomindang in 1949 were the features that in the new post-1949 context, allowed the ruling party to so abuse its power that the foundations of regime legitimacy were severely undermined. In the wake of Mao's passing in 1976, the Deng Xiaoping leadership has sought to rebuild the regime's legitimacy on the basis of the principles of popular support, "scientific laws" that limit party action by providing social groups with areas for autonomous action and with legality.

The volume consists of eight chapters and an introduction, the earliest of which dates from 1967 and the most recent of which was written in the mid-1980s. All but the useful introduction and the concluding chapter first appeared in other publications. In bringing these pieces together under a single cover, Tang Tsou has rendered scholars both inside and outside the China-studies field a great service. Though the range of specific topics is great—social crisis and reintegration in twentieth-century China, the origins and dynamics of the Cultural Revolution, the role of informal groups in Chinese politics, Mao's Sinification of Marxism and the use to which the "gang of four" put ambiguities in that ideology, the post-Mao retreat from ever increasing totalitarian penetration of society, the responsibility system in agriculture, and what might be called "constitution building" in the post-Mao era—there are at least three threads that run throughout the fabric of this volume and throughout Tang Tsou's highly productive career.

The first theme is the need for research on Chinese politics to be explicitly comparative and informed by the broader theory-building concerns of social science. "The essays attempt to undertake 'a theoretically informed and theoretically relevant case study' of the Chinese Communist movement and regime" (p. xvii). Therefore, the chapters address issues of universal concern: state building, state-society relations, the implications of institutionalization for revolutionary ideology, the conflicts that arise between charismatic leaders and routinizing bureaucracies, the meaning of totalitarianism in the specific context of post-1949 China, the role of informal groups in policy making, and the importance of ideology in mass and elite conflict and in policy making.

The second recurring theme is that the political and social issues of today cannot be understood without reference to their historical evolution and the development of the political and administrative institutions charged with handling these issues. For Tang Tsou, as for Richard Neustadt and Ernest May, "time is a stream." Throughout the book, one sees the enduring dilemmas confronting Chinese rulers and political theorists: What is the proper relationship between central control and local autonomy? To what degree should political leaders be constrained by constitutional and legal bonds? Can, and should, Chinese political thinking place more importance on the concept of "citizenship" and less on the notion of "the masses"? Where is the "middle way" between the pragmatism implied by the slogan "practice is the sole criterion of truth" and the demands of a Leninist party that the party remain unchallenged?

The final thread is the *hope* that although the course of modern Chinese political development has been circuitous and costly, the Chinese polity, in its own way and in its own time, is gradually establishing a stable basis for political legitimacy, that this polity will be more liberal than its predecessors, and that it will be capable of producing economic progress. Each stage has contributed something to this evolution, even the Cultural Revolution. In the case of the Cultural Revolution, for example, Tang Tsou argues that the chaos at least served to make clear to all Chinese the dangers inherent in the totalitarian ambitions, and feudal underpinnings, of the late Maoist state. It has been this societal learning experience that, in part, accounts for the post-Mao retreat from "'revolutionary-feudal' totalitarianism."

Tang Tsou fervently hopes that "the process of routinization may finally have set in" (p. 334) after thirty-some years, but he is by no means sure. Decisive in determining the fate of this effort will be elite cohesion in Beijing, the depth of social cleavages resulting from current development policies, and the nature of the international environment.

DAVID M. LAMPTON

*Ohio State University*

**Military Rule in Chile: Dictatorship and Oppositions.** Edited by J. Samuel Valenzuela and Arturo Valenzuela (Baltimore: Johns Hopkins University Press, 1986. xi, 331 p. $35.00).

This volume makes accessible to an English-speaking audience nine original essays on Chilean authoritarianism. Three of them, by

Alejandro Foxley, Ricardo Ffrench-Davis, and Pilar Vergara, examine the major shifts in social and economic policy that followed the 1973 Chilean coup. Three others analyze political opposition and resistance to military rule. These include a chapter by Brian Smith on the church, a coauthored essay by the editors on party oppositions, and an analysis of the labor movement by Manuel Barrera and J. Samuel Valenzuela. The remainder of the book, which consists of chapters authored by Genaro Arriagada, Manuel Antonio Garreton, and Heraldo Munoz, examines the legal and institutional framework of the armed forces, the dynamics of institutionalization and opposition between 1973 and 1980, and foreign relations. All of the essays are empirically focused, analytical and dispassionate in tone, and ably translated. The resulting volume, which is the most comprehensive overview of Chilean authoritarianism to have been published to date, speaks well not only for the editors but for the sophistication and continuing vitality of Chilean social science.

The usual complaints about a book of this type are uneven quality and incoherence. In this instance neither criticism is fully appropriate, although certain essays do stand out from the rest of the collection. It is also true that the editors might have done more to pull the individual contributions together. A skimpy introduction provides no theoretical framework, nor is there any conclusion reflecting upon what this collaborative exercise has added to our understanding of Latin American authoritarianism. In their brief foray into the theoretical realm, the editors focus mainly on the issue of regime definition, arguing that the Pinochet regime should be thought of not as fascist but as an "authoritarian situation" or "a case of failed bureaucratic authoritarianism" (p. 5). This characterization sits uneasily with other contributions to the volume, especially Garreton's inspired analysis of the dynamics of regime institutionalization. The editors' emphasis on the fragilities and failures of the regime, particularly its lack of institutionalization and legitimation, also seems misplaced given the unusual durability of military rule in Chile as compared to other southern-cone nations. Failures and fragilities provide no basis for understanding this durability, nor does "Pinochet's proven capacity to command the armed forces" (p. 10) fully explain why Chilean authoritarianism withstood the continentwide democratization trend of the early 1980s. Certainly, coercive capability is critical, but other factors warrant emphasis as well.

The theoretical weaknesses of the introduction characterize the volume as a whole. The Chilean case is not adequately placed in comparative perspective, nor do the regime's strengths receive sufficient attention. The volume provides us with three chapters on opposition forces, but only one, which deals exclusively with the army, looks at regime elements. Thus the functioning of the government, the composition of the ruling coalition, the regime's base of social support, intraregime tensions, and so forth, all remain rather obscure.

Perhaps the most regrettable feature of the volume is its publication date. All of the essays were originally prepared for a 1980 Wilson Center workshop, "Six Years of Military Rule in Chile." Despite subsequent revision and updating, the central focus remains the 1973-80 period. Indeed, only two of the nine chapters even refer to the profound economic and political crisis of 1983. Consequently, there is no analysis of significant recent developments, such as the resurgence of popular opposition or the emergence of divisions within the governing coalition. Particularly as the editors asked the contributors to emphasize description rather than theory, the result is a volume that was rather dated by the time of publication.

Events have overtaken the publication of this collection in a second sense as well. In 1980 the papers written by the contributors were fresh, and they significantly expanded our understanding of the Pinchot regime. Six years later the situation was rather different, inasmuch as most of the contributors had by then published extensively on their chosen topics. The importance of Arriagada's chapter on the military, for example, pales considerably in comparison with his two pathbreaking books, *El pensamiento politico de los militares* (1981) and *La politica militar de Pinochet, 1973-1985* (1985). In short, today the book is anything but a pioneering volume.

Despite these limitations, the collection represents a major addition to the English-language literature on authoritarianism and Latin American politics. It distills some of the best research that has been done over the past decade on Chilean politics. Because of the unusually high quality of the individual contributions, most chapters would make excellent assigned reading for both undergraduate and graduate courses. Comparativists wishing to familiarize themselves with Chilean developments during the first years of the Pinochet regime will also find this a valuable resource.

KAREN L. REMMER

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**National Styles of Regulation.** By David Vogel (Ithaca, NY: Cornell University Press, 1986. 328 p. $39.95, cloth; $12.95, paper).
**Politics of Health and Safety.** By Graham Wilson (Oxford: Oxford University Press, 1985. 179 p. $19.95).

During the "environmental decade" of the 1970s, most advanced industrial states dramatically stepped up their efforts to protect public health and the environment against risks created by hazardous technologies. Students of comparative policy reacted rather slowly to this expanding sphere of state activity, but the picture has now changed. In the past six years there has been a minor explosion of works comparing health, safety and environmental policies in Europe and the United States. Most of these studies, including the works by Vogel and Wilson, have concluded that the U.S. approach to regulating technology differs sharply from that of any other country at a similar stage of economic and technical development. Looking respectively at environmental protection and occupational safety and health, Vogel and Wilson document particularly striking divergences in policy approach between Britain and the United States. Whereas U.S. agencies have opted for formal procedures, rigid legal standards, wide public participation, and strict enforcement, British agencies have consistently favored an informal approach, characterized by flexible guidelines, limited consultation, and voluntary compliance. As a result, regulation has generated far more intense political conflict in the United States than in Britain. Yet these two books concur with other recent cross-national studies of regulation in concluding that U.S. policies have not proved notably more effective than their British counterparts. In short, while the tangible and intangible costs of regulation seem to have been higher in the United States, the U.S. public does not appear to have reaped commensurate benefits in terms of safer workplaces or a cleaner environment.

David Vogel's book contains the most ambitious attempt to support this conclusion in the existing literature on comparative regulatory policy, but the unsystematic character of his comparisons induces skepticism. Vogel recognizes the methodological problems. As he notes, "measuring the impact of regulation on environmental quality is extremely difficult within a given political system, let along cross-nationally" (p. 147). Comparisons between Britain and the United States, in particular, are hampered by the fact that the two countries have frequently established different policy priorities, so that gains made in one country are not closely paralleled in the other. Vogel has no solution to this classic apples-and-oranges problem of comparative policy studies. We learn in chapter 4, for example, how successfully Britain has dealt with smoke pollution from domestic fuel combustion and what progress the United States has made in abating motor-vehicle emissions but not how these two achievements stack up against each other. This shortcoming reflects a serious structural flaw in an otherwise admirably researched and readable work. The first part of the book is not genuinely comparative, but rather "an examination of British environmental policy as seen through the eyes of a student of American politics" (p. 11). Comparable detail for the U.S. case might have made the book unwieldy, but its absence deprives the author of a systematic basis for comparing policy outcomes in the book's second part.

Graham Wilson's book is a much slighter work than Vogel's, and it suffers from careless editing (e.g., "prize mechanists" for "price mechanisms," p. 6), as well as a tendency to confuse British and U.S. usage (e.g., "Courts of Appeal" for "Courts of Appeals," p. 92). Nevertheless, it provides a useful counterpoint to the longer study and avoids some important methodological pitfalls. Working on a more manageable canvas than Vogel, Wilson finds it easier to establish valid points of comparison between British and U.S. policies and their consequences. He argues that the conciliatory British approach to regulation has done more to reduce industrial injury rates in the 1970s than the adversarial strategy employed by Occupational Safety and Health Administration (OSHA). With respect to occupational health, however, neither country seems to have proved particularly successful, at least when judged by the number of health standards each has issued. This is an intriguing convergence, since health concerns, such as carcinogens in the workplace, have received higher priority on the U.S. regulatory agenda than the British. The evidence Wilson provides, however, remains suggestive rather than conclusive. A rigorous comparison of the content, timing, scope and enforcement of the health standards issued in each country would have helped strengthen the claim that, as in the environmental area, Britain has achieved no less than the United States and at lower political cost.

In explaining the relative lack of success of U.S. social regulation, both Vogel and Wilson lay primary blame on the conflict-ridden nature of the U.S. decision-making process, which pits regulators against regulated interests at every turn. Their books present a compelling case that cooperation between business and government in developing regulatory policy is not synonymous with capture, and that, when certain preconditions are met, Britain's style of reasoned persuasion is likely to produce better policy results than the U.S. alternative. In general, the British approach seems to work best when the hazards to be controlled are scientifically well understood and their dimensions clearly defined. But both authors agree that cooperation is less fruitful with respect to the indeterminate, scientifically controversial risks that have occupied a central place on the U.S. policy agenda. Occupational health hazards are a prime example. As Wilson observes, both political systems have failed with this issue, though the failure has taken different forms: "pluralist deadlock in the USA and corporatist deadlock in Britain" (p. 169).

In both works, comparative findings provide a point of departure for testing prevailing hypotheses about the relationship of policy and politics. The authors agree that the striking differences in regulatory style exhibited by Britain and the United States rule out a strong form of the "policy sector hypothesis." In environmental protection as in occupational safety and health regulation, similar types of policy have not produced similar types of politics. Vogel, however, notes that comparative analysis does not unequivocally support the view that "politics matters" (p. 222). One must still explain why, despite their differences in regulatory style, Britain and the United States have converged not only in policy outcomes, but also, as Vogel believes, in their definition of the policy agenda. The latter observation seems problematical in light of the evidence in chapter 4 of Vogel's book that each nation has established its own distinctive priorities for environmental protection. Without resolving the contradiction, however, Vogel concludes that convergences in policy agendas and outcomes are the result of common socioeconomic variables, specifically, similar levels of industrial development in the two countries and the influence of international communication.

This analysis implies that in each country it is the "style" of regulation, rather than policy goals or outcomes, that is most resistant to forces of economic and technological determinism. Vogel advances political culture as the reason for this phenomenon. In an interesting historical exploration, Vogel traces the dynamics of British and U.S. business-government relations over more than a century in order to show that current policies and procedures are rooted in past traditions. In the British case, there is impressive evidence that the preference for consensus over conflict dates back more than a hundred years to mid-Victorian reforms of public administration. Vogel argues that these reforms withstood the test of time because Britain at the end of the nineteenth century "witnessed an important cultural backlash against the values of industrial civilization" (pp. 242-43). Accordingly, "business values" never gained the upper hand in Britain to the extent they did in the United States. An un-self-confident business community remained deferential to public authority while the civil service grew in status and prestige; correspondingly, the public never became unduly suspicious of the power of business. In the United States, on the other hand, there were no moderating influences such as the British aristocracy to check the political power of large corporations. As their influence grew, so too did public mistrust of business. This hostility and lack of trust, according to Vogel, fueled the regulatory initiatives of the 1960s and 1970s, accounting for the confrontational thrust of environmental policy in this period.

The trouble with this explanation is that recent conflicts over social regulation in the United States seem much more a sharp break with than a natural continuation of past relationships between regulators and the regulated. Vogel acknowledges, for example, that the style of economic regulation in the United States has generally been highly cooperative. Wilson rejects the political-culture explanation on these grounds, observing that U.S. culture "never seemed a barrier to the capture of regulatory agencies to writers before the 1970s" (p. 153). Moreover, Wilson cautions against exaggerating the tendency to compromise in British culture, noting that it has not carried over into neighboring areas of industrial relations. According to Wilson, experience rather than culture explains many of the differences between the two countries. The longer history of health and safety regulation in Britain produced a series of mutual adjustments between business and government that have not had a chance to occur on the U.S. scene, where federal control of policy is much more recent. Further, Wilson suggests that OSHA's coercive style of regulation reflects a deliberate political choice by Congress, which in 1970 wanted to create an agency that would be maximally resistant to capture. The legislature thus conferred powers on OSHA in a way that inhibited the development of smooth working relations between the agency and those it regulated.

None of this holds much comfort for U.S. policy makers seeking fruitful avenues of regulatory reform. The exceptionalism of the U.S. approach, which both studies confirm, reduces the possibility of U.S. agencies learning from other political systems. Vogel suggests, moreover, that a cooperative approach would not have proved workable in the United States and that an adversarial style is better suited for regulating a business community that is "extremely competitive, jealous of its prerogatives, and contemptuous of government officials" (p. 259). Wilson believes that the kind of authority a regulatory agency needs in order to make the cooperative approach work effectively is impossible in the United States "given the multiplicity of channels . . . open to anyone who wishes to challenge an agency" (p. 167). These pessimistic assessments, however, are based largely on research focusing on the 1970s. Neither work casts much more than a passing glance at U.S. regulation under Reagan, though arguably the style of policymaking in this period has moved closer to the cooperative British model. At the same time, Britain, as Vogel notes, has come under pressure from Europe to make its regulatory policies more formal and legalistic and pressure from domestic interest groups to create a more open and participatory regulatory process. These developments may lead to interesting convergences between Britain and the United States at some future date. In the meantime, it would be well to withhold a final verdict on the relationship between regulatory policy and political culture.

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# INTERNATIONAL RELATIONS

**Security or Armageddon: Israel's Nuclear Strategy.** Edited by Louis Rene Beres (Lexington, MA: Lexington Books, 1986. x, 242 p. $25.00).

**Israel's Global Reach: Arms Sales as Diplomacy.** By Aaron S. Klieman (McLean, VA: Pergamon-Brassey's International Defense Publishers, 1985. xiii, 241 p. $22.50).

*Israel's Global Reach* and *Security or Armageddon* both deal with critical issues related to Israel's national security. The former is a study of Israel's arms-transfer policy, while the latter is a study of Israel's nuclear policy. The first deals with the proliferation of conventional weapons, the second with the proliferation of nuclear weapons. *Israel's Global Reach* addresses horizontal proliferation; *Security or Armageddon* emphasizes vertical proliferation. The former deals with *exportation* of arms from Israel, the latter with *importation* and *development* of knowledge, material, and capabilities in Israel. Both books see Israel's security policy as indistinguishable from its foreign policy. They both identify the motives and incentives for Israel's conventional and nuclear policies to be primarily security-related. Both books analyze a case study of important regional and global implications. Beyond that, however, the two books differ markedly: Klieman's book attempts to show how Israel has developed a global reach through the combination of arms sales and diplomacy. Beres's collection of 12 essays attempts to evaluate Israel's nuclear-policy choices and the probable impact of those choices.

To achieve his goals, Klieman briefly describes the international arms market and "Middle Eastern realities" (chapter 1), Israeli defense sales in historical perspective (chapter 2), security motives and diplomatic incentives (chapter 3), and the economic rationale for arms sales (in chapter 4). He then identifies and describes the major defense industries (chapter 5). He explains how arms-transfer policy is made in Israel (chapter 6), describes Israel's worldwide sales campaign (chapter 7), its special military relations with the so-called pariah states (chapter 8) and with the United States (chapter 9). Finally, he presents a number of prospective challenges to Israel's further "ascendance" in the area of conventional weapons transfers (chapter 10), and several guidelines as to how Israel should combine "maximum effectiveness with minimum risk in its weapons transfers practices during the remainder of this decade" (p. 216). According to Klieman, this is "as much a study of Israeli foreign relations as it is of Israeli arms trading" (p. xi).

Klieman's book is a highly informative, carefully researched and well-documented study based on a variety of sources (local and foreign, formal and informal, academic and nonacademic). Klieman successfully shows that arms sales represent a central component of Israel's *external* relations, defense posture, and foreign trade. What is clearly missing in this book, however, is an analysis of the impact of arms production and sales on Israeli society and politics. The rapid development of an arms industry in Israel has brought about a structural change in its economy and a transformation of its value system and has a profound impact upon the moral direction of Israel's society. These should have been addressed as thoroughly as the theoretical implications for civil-military relations and for trends towards militarization of Israeli life. Nevertheless, *Israel's Global Reach* is a very valuable contribution to the study of arms transfers.

Beres's book deals with Israel's nuclear dilemmas: ambiguity versus disclosure and the choice between covert and overt nuclear postures. As Beres himself so eloquently puts it: "If there is a 'bomb in the basement,' an undisclosed force of nuclear weapons or components that could be assembled rapidly into such weapons, should it remain there? Or would Israel be better served by moving beyond its current policy of deliberate ambiguity to one of an explicit declaration of nuclear capability? If, as some scholars suggest, Israel has never actually moved to implement a nuclear weapons option, should it now exercise that option? And if it does, should the necessary steps be taken under the mantle of obscurity, or should they be accompanied by public disclosure?" (p. 1).

These policy options are examined by the various contributors to the book in a variety of ways, ranging from the analysis of the costs and benefits associated with each option (Dowty, Steinberg), the assessment of the nuclear threats versus the conventional threats facing Israel (Eytan), an evaluation of the probability of nuclear terrorism (Friedlander), a reappraisal of the efficacy of Israel's conventional deterrent (Yaniv), the legal aspects associated with a policy of "going nuclear" (Weston) and the moral dilemma involved (Cohen). There is a broad consensus among contributors to this volume that Israel's security will be better served by the continued pursuit of a policy of deliberate ambiguity and that the superpowers "balance of terror" is an ill-conceived analogy to the volatile Middle East arena.

While each of the articles in the volume carefully analyzes Israel's strategic choices, there is a considerable amount of overlap across the essays; for example, regarding the bomb-in-the-basement argument and the history of the Israeli nuclear policy, one wonders why the editor allowed so much repetition instead of encouraging debate on more original issues, such as that presented by Stephen Cimbala in his chapter on implications for Soviet-U.S. strategic policies or by the proposal for a regional nonproliferation treaty for the Middle East (Avi Beker). Perhaps the emphasis on the "deliberate-ambiguity" policy advocated in this book comes in response to the argument, raised over the last decade by a number of analysts (e.g., S. Aronson, S. Rosen, and R. Tucker) and elaborated in a recent book by Shai Feldman, *Israeli Nuclear Deterrence: A Strategy for the 1980s* (New York: Columbia University Press, 1982), that a disclosed Israeli nuclear-weapons status might stabilize the Middle East. Despite this, however, Beres's book constitutes an important contribution to this ongoing debate.

ALEX MINTZ

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**The Territorial Dimension of Politics: Within, among, and across Nations.** By Ivo D. Duchacek (Boulder, CO: Westview, 1986. xvi, 328 p. $35.00).

The territorial state has found a friend in Professor Ivo Duchacek. He defends it against reformers who for the sake of peace, development, and justice would tear down its borders to make room for regional communities or a global order. He documents, and seems to relish, the reformers' failure to remove the territorial state from the center of the international system. The territorial bond, he observes, whether rooted in instinct, language, habit, duty, coercion, and/or indoctrination, has not withered in the face of assaults from universal creeds. He notes the paradox whereby such creeds promote from within nations worlds without nations, and spread their dogmas from one nation to another. Universal Marxist premises, he writes, have been distorted to accommodate the durability of territorial identities. Contrary to Marxist expectations, international proletarian loyalties have not ushered the demise of territorial boundaries; instead, proletariats have been organized along territorial lines. Today the communist movement comprises distinct, self-interested states, each with its own territorial personality.

While Duchacek sees no signs that the approximately 160 territorial "corrals" are about to be replaced by supranational regionalism or globalism, he does recognize that growing worldwide interdependence is transforming the nation-state. National boundaries, he tells us, are becoming porous, permitting the flow of more people, ideas, goods, and pollutants. Duchacek also stresses how in its international relations, the territorial state has become more decentralized, speaking to the world not in one but in many voices, as subnational governments such as the Canadian provinces and the U.S. states bypass their national institutions to establish direct economic and diplomatic ties with other governments. The independence of such subnational governments as Quebec, which has important links with various U.S. states and foreign countries, as well as a special relationship with France based on a common linguistic and ethnic heritage, leads Duchacek to label national boundaries as "meshlike." Thus the territorial state, although alive and well, is certainly not a homogeneous monolith.

Duchacek's assessment of territorial politics, which is informed throughout by a healthy skepticism toward elegant prescriptions for the inelegantly complex world of international politics, is eminently reasonable; so reasonable, in fact, that an educated person who regularly reads a good newspaper would more than likely reach the same conclusions. The assiduous reader who gets through the book's nearly three hundred pages of dense prose will not find much more than this reasonable assessment to reward the effort.

The book is not more rewarding, in part, because it covers so many subjects—the list is long and eclectic—that it covers few well. What can we learn about how instinct affects territorial bonds or how transnational corporations erode national sovereignties when these subjects are examined in fewer than two pages? There are also many general unsubstantiated observations. He states, for example, that exchanges across borders have become more common, but he says little else about the volume and character of these exchanges.

The book also disappoints by not addressing adequately the question that gives its subject political significance. Are the reformers right when they warn that the persistence of territorial fragmentation will spell disaster in the form of nuclear war, population explosion, resource depletion, or environmental decay? Duchacek thinks not. He categorically rejects the notion that humankind is doomed unless our territorial corrals are discarded. This is a sensible position, but I find very little in the book in the way of argument or evidence that would help me to defend it.

Likewise, he does not evaluate how the growing autonomy of subnational governments affects international politics. He first warns us that it may foster strains and confusions among nations. And then he states that it is neither a blessing nor a curse, but a fact of international politics that will not go away.

Duchacek does offer a useful antidote to naive ideologues who believe that the abolition of the world's nations is a feasible way to resolve the world's problems. It is a pity, however, that he does not offer much more.

JEFFREY OBLER

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**The Struggle for the Third World.** By Jerry F. Hough (Washington: Brookings Institution, 1986. x, 293 p. $32.95, cloth; $12.95, paper).

**Soviet Power and the Third World.** By Rajan Menon (New Haven: Yale University Press, 1986. ix, 261 p. $20.00).

Superpower policy in the Third World has attracted increasing scholarly attention over the last decade as spokespersons for the major competitors have identified less-developed countries as crucial to the "world correlation of forces" (Moscow) or the "world balance of power" (Washington).

Rajan Menon of Lehigh University and Jerry Hough of Duke University and the Brookings Institution both conclude in their new books that the realities of international relations have turned superpower rivalry toward competition in the Third World but that the complexity of politics in those areas has defied the expectations of policy makers on both sides.

These books are policy-oriented, and a reassessment of superpower policy toward the Third World is certainly timely. But, ultimately, both authors are mainly concerned with U.S. policy while their research is concentrated upon viewpoints expressed by Soviet scholars and officials below the top policy-making level. Accordingly, the wide-ranging criticisms of U.S. policy expressed by the authors do not necessarily follow from the research.

Both Menon and Hough concede that Soviet scholars and officials work within a prescribed ideological framework but maintain that the ideological component of foreign policy analysis is much less fixed than most Westerners assume. Menon covers a wider range of Soviet analyses than does Hough, but the broad sweep of his coverage necessarily yields less depth in his examination. However, Menon also produces a wealth of data on actual Soviet interaction with the Third World.

Menon deals in detail with economic exchanges, Soviet power-projection capabilities, and arms transfers; on the third of these foreign policy tools, his research is exceptionally thorough. On the basis of this extensive review, Menon presents a view of the Soviet Union as much less threatening to Western interests than leading U.S. spokespersons claim. However, one instrument of Soviet foreign policy is ignored. As Steven R. David has pointed out, USSR and USSR-surrogate security forces constitute an important factor in the consolidation of Soviet footholds in the Third World, and there is no hint of this in Menon's book.

Menon's *bete noire* is U.S. Secretary of Defense Caspar Weinberger, who is viewed as having a simplistic, unrealistic view of Soviet global aims. Major Soviet gains in the Third World are attributed to "advantageous nonmilitary circumstances"; the Soviets, as a world power, naturally move to take advantage of such situations. But the conservative Western critique of Soviet policy emphasizes that the practical problem is precisely the existence of power "vacuums" that can be filled by the Soviets, changing the world balance of power (Weinberger himself is quoted by Menon on this point). Menon seems to be arguing that the USSR should not be considered such a serious threat because it only acts to fill such power "vacuums" and that U.S. policy designed to foreclose such moves represents overreaction. But authoritative statements by leading Soviet political figures in the 1980s on the "correlation of forces" indicate that changed U.S. policy on these power "vacuums" is considered by them to be crucial. Menon acknowledges that there is a less favorable "correlation of forces" for the Soviets in the 1980s but does not credit U.S.

policy for this.

Hough's book is devoted to the writings of Soviet scholars and he demonstrates considerable divergence in their views on Third World questions. Hough's command of Soviet sources is unparalleled and his meticulous research can only evoke admiration. The author points out that the importance of Soviet institute scholars lies in the indirect, behind-the-scenes influence they can exert. One spectacular coup in his research tends to bear out this contention.

The major debate on foreign policy between what Dan and Rebecca Strode called the "unilateralists" and the "diplomacists" in 1983 attracted much attention in the West. Hough demonstrates that this debate was being carried on at lower levels a decade earlier. Moreover, Hough raises the prospect that foreign-policy analysts with more flexible, more pragmatic viewpoints may emerge with greater authority as Gorbachev restaffs the Soviet bureaucracy. The rise of Aleksandr Iakovlev is noted as evidence of such a tendency. Hough's argument for a more restrained Third World policy by the U.S. hinges partially upon this possibility. The jury is still out on this point and skeptics will note the mixed record on Hough's past projections of favorable outcomes for Soviet "reformist" elements.

The policy views of these authors will arouse much controversy, but it must be acknowledged that the excellence of research and the positing of important questions make both volumes required reading.

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**Persistent Patterns and Emergent Structures in a Waning Century.** Edited by Margaret P. Karns (New York: Praeger, 1986. xii, 311 p. $38.95).

**Neorealism and Its Critics.** Edited by Robert O. Keohane (New York: Columbia University Press, 1986. x, 378 p. $45.00, cloth; $13.00, paper).

Since the publication of Kenneth Waltz's chapter in the *Handbook of Political Science* edited by Fred Greenstein and Nelson Polsby (Reading, MA: Addison-Wesley, 1975), a small but prominent group of U.S. political scientists has debated the merits of what is termed a *neorealist* analysis of international politics. *Neorealism and Its Critics* is a collection of contributions to this debate. Selections by Waltz, Robert Keohane, John Ruggie, Robert Cox, Richard Ashley, and Robert Gilpin have been previously published as journal articles or book chapters. An introductory chapter by Keohane, a postscript by Robert Cox to his 1981 article, and Waltz's concluding chapter in which he responds to his critics are published here for the first time.

Its proponents and many critics agree that neorealism has much in common with the older realist approach. As Robert Keohane notes, neorealism can best be understood as an attempt to "systematize political realism into a rigorous, deductive systemic theory of international politics" (p. 15). What is novel about Waltz's approach, as well as that of Robert Gilpin in *War and Change in World Politics* (New York: Cambridge University Press, 1981), is the reliance on 1950s-style microeconomic theory as the formal basis for a substantively realist theory. Many of the criticisms of neorealism presented here do not differentiate between realist and neorealist positions, but some critics (most consistently, Keohane) respond to the explicit reliance on microeconomics by an equally explicit attack on this strategy. This aspect of the debate raises many points that are relevant to a broad variety of rational-choice models of political behavior, and it deserves a wide audience.

Broader concerns about the limitations of realist arguments lead to many criticisms that are quite cogent if not necessarily novel. Neorealists Waltz and Gilpin agree with their critics that their theories at best delineate a set of constraints on state action, but more precise predictions require a theory of the state or attention to information processing and decision making. They differ with contentions that neorealism cannot explain long-run changes in the nature of the international system, arguing that in principle these inquiries are entirely consistent with their approach. However, they still treat endemic international conflict as an assumption rather than as a phenomenon that merits explanation (p. 304). As a result, the question raised by Richard Rosecrance in *The Rise of the Trading State* (New York: Basic Books, 1986)—whether the drive for territorial expansion is a waning force in international politics—must receive an a priori negative reply.

The most remarkable part of Waltz's reply to the critics is his virtual abandonment of microeconomics as the formal skeleton for neorealism. Rationality now denotes merely sensitivity to costs, and state behavior is now seen as shaped by natural-selection processes operating on behaviors (pp. 330-31). This is simply another way of talking about operant conditioning, and some of Waltz's comments on the international system suggest that he views it as a sort of Skinner box (p. 337). Adaptive rationality has thus replaced synoptic rationality, with uncertain consequences for the whole neorealist project.

One major disappointment is the relative lack of attention by either proponents or critics to ongoing research programs that begin either from neorealist or from alternative premises. Conceptual criticism has its value, but surely there is much to be gained by evaluating the results of actual research.

The papers collected and edited by Margaret Karns in *Persistent Patterns and Emergent Structures in a Waning Century* provide some indication of the impact of the neorealism debate on patterns of research. The papers were taken from the 1985 International Studies Association meeting, which had as its theme the title of this book. A number of papers (those of Richard Mansbach and Yale Ferguson, David Wilkinson and Arthur Iberall, Glenn Paige, and Morton Deutsch) are critical of realist/neorealist thinking, and they do cite empirical research in support of their criticisms. However, these authors generally do not present original theorizing or empirical findings. Two papers by Jack Levy and Clifton Morgan phrase questions from a realist perspective, but their findings could certainly be accommodated within other views as well. The remaining papers (by Ted Gurr, Robert Wells, Michael Intriligator and Dagobert Brito, John Ruggie, Thomas Cusack and Barry Hughes, and Phillips Cutright and Herbert Smith) cannot be neatly classified as realist or antirealist.

Many of the papers provide extensive summaries of the literature on a given topic or survey the state of the discipline. Of the remaining papers, the one most relevant to identifying "emergent structures" is Thomas Cusack and Barry Hughes's simulation of future government-military-expenditure patterns using their computer model.

Given the abstract nature of the theme of "persistent patterns and emergent structures" and the diversity of research interests within the U.S. international-relations scholarly community, an anthology such as this could have been just another hodgepodge of highly specialized articles. Editor Karns is to be congratulated on avoiding this outcome by selecting a range of papers that will appeal to a broader readership.

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**Dynamic Models of International Conflict.** Edited by Urs Luterbacher and Michael D. Ward (Boulder, CO: Lynne Rienner, 1985. xiv, 561 p. $38.50).

At least since the time of Thucydides, who argued that the *growth* of Athenian power was a major factor leading to the Peloponnesian war, it has been recognized that an understanding of international conflict requires an appreciation of the dynamic forces at work in the international system. Although the importance of these forces has long been accepted by international-relations scholars, it is only in recent years that a great deal of intellectual effort has been devoted to the construction of formal models of the dynamic processes involved in international conflict. Beginning with the work of Richardson, who utilized differential equations to model arms races, the field has witnessed an impressive growth in the use of dynamic models in the analysis of conflict behavior. One weakness in this body of research is that there have been too few attempts at joining the various strands of theory that have been developed.

In the autumn of 1984, Urs Luterbacher and Michael Ward sought to address this problem by organizing a conference that brought together a number of scholars who have used a variety of dynamic models in analyzing various aspects of international conflict. *Dynamic Models of International Conflict* is the product of that conference and contains the papers presented by the participants. The contributions address a wide range of conflict processes, including arms races, initiation and escalation dynamics, and trends in foreign-policy behavior (using events data) and demonstrate the usefulness of a number of models, such as game-theoretic models, differential-equations models, and "politometric" models. Many of the chapters consist of summaries or illustrations related to prior research, so one should not read this volume expecting to find new developments in a familiar area. The book will be useful to those seeking an introduction to this body of research, and specialists will be interested in those papers focusing on relatively new developments (such as artificial-intelligence methods or the computer-graphics display used by Ward and House) and those advocating wider use of methods generally overlooked by international-relations scholars (e.g., transform methods). While many of the papers are probably too technical for the book to serve as an introduction to those completely unfamiliar with this type of research, it is a useful "handbook" for specialists in the field.

Although the research methods and topics represented in the collection of articles are clearly related, little effort is made to pull the studies together into a coherent whole. There are no section introductions and no conclusion and the book's introduction is too brief to provide any synthesis. While most readers should be able to perform this task for themselves, the purpose of the book would have been well served by the inclusion of a chapter showing how the individual studies jointly contribute to our overall understanding of international conflict. The volume would also have benefited from the inclusion of a number of critical chapters. Some efforts at criticism have been made (Schrodt's analysis of rational models is the most interesting) but these are generally brief and are incorporated into introductions for alternative models. As it stands, the book is a collection of articles representative of much of the research being conducted on the dynamic processes of international conflict, but there is no effort to join the strands of theory or even to provide constructive criticism to guide future research.

One annoying problem is that there are many typographical errors in the book. I usually find comments of this type rather petty since misspelled words are usually only a minor irritation, not a major drawback. In this case, however, the problem is more serious. There are errors in a number of the formal equations as well as in the text. Some are relatively insignificant (such as the omission of a bracket) but several have a major effect on the authors' arguments. For example, at one point the payoffs for a prisoners'-dilemma game are misspecified (p. 55) and at another the Laplace transform of a function's second derivative is given as

$$\left[\frac{d^2f(t)}{dt^2}\right] = s^2L_f - sf(0) - f(0)$$

rather than as

$$L\left[\frac{d^2f(t)}{dt^2}\right] = s^2L_f - sf(0) - f'(0)$$

(p. 179). These types of errors make some of the arguments difficult to follow and lead to a tendency on the part of the reader to expend little effort on difficult passages because one can assume that misprints will create a significant barrier to understanding.

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**Imperialism and After: Continuities and Discontinuities.** Edited by Wolfgang J. Mommsen and Jürgen Osterhammel (London: Allen & Unwin for the German Historical Institute, 1986. xii, 368 p. $29.95).

Until recently, there has been surprisingly little effort to integrate the work of historians examining the history of Western imperialism with that of social scientists studying contemporary North-South relations. *Imperialism and After* represents an ambitious effort to bridge this gap by bringing together the work of a number of prominent historians, economists, sociologists, and political scientists to explore continuities and discontinuities between the imperialist domination of much of what is now called the Third World in the nineteenth and early twentieth centuries and the external relations of the independent nations that have emerged from the decolonization process of the postwar period. Its 22 contributions, which were first presented at a 1982 conference sponsored by the German Historical Institute, London, offer sophisticated analyses of some of the most contentious and difficult issues of contemporary-development studies.

It is difficult to summarize as long and wide-ranging a book as this in a brief review. Several themes do, however, run through most of the essays. First, nearly all of the scholars represented in the volume agree that formal imperialism is only one manifestation of a broader phenomenon of dominance and dependence that also includes informal means of control. There is less agreement as to exactly how *informal imperialism* should be defined, and in particular on where one draws the line between it and the ordinary exercise of power among unequal actors in the international system—that is to say, the theory of international politics as a whole. To the extent that there is agreement among the authors represented here, it is that a relationship of imperialism is more profoundly asymmetrical and intrudes more extensively into a subordinate country's domestic political and economic system than ordinary power politics. But, as a number of contributors point out, the line between imperialism and power politics is in practice not an easy one to draw.

A related point of agreement among the contributors is that imperialism cannot be fully understood unless adequate attention is given not only to its sources but also to its interaction with domestic power structures in the subordinate country, a process in which imperial powers both influence and are influenced by local politics. Here the central theoretical contribution is Ronald Robinson's "excentric" theory of imperialism. Robinson argues that "European imperialism generated its main force through linking and exploiting relative inequalities in local and regional balances of wealth and power" and that as a result, "the nature and extent of imperialism, with or without empire, can only be defined in terms of local collaborative systems at the point of impact" (pp. 272-73). Several excellent case studies depict this process, most notably Albert Wirz's comparison of French and British colonialism in Chad and Nigeria, Dieter Brotel's examination of French and U.S. domination of Vietnam and Cambodia, and David Fieldhouse's historical analysis of the changing character of relations between multinational corporations and Third World states.

In addition, considerable attention is given in the volume to the longstanding debate over whether modern imperialism is primarily economic or political in character—a debate that hearkens back to Hobson, Lenin, and Schumpeter. Although many of the authors emphasize economic ties between developed and underdeveloped areas, a number of essays demonstrate that the political, cultural, and ideological origins of imperialism at times clearly predominate over economic considerations. Thus Bernd Martin concludes that the impetus for Japanese imperialism of the 1930s and 1940s can be traced to a "mythical nationalism, which was rooted in the peasant masses, but could also be manipulated from above" (p. 66) and which arose from the strains of rapid industrialization. Hartmut Pogge von Standmann identifies the German imperial movement of the interwar years with a yearning for restoration of Great Power status, and Klaus Schwabe and Tony Smith consider U.S. foreign policy in the postwar period to be the product more of political and ideological, than of economic, motivations. Needless to say, the collection offers no definitive conclusion to this longstanding debate, but the case studies offered should certainly give pause to anyone inclined to accept a reductionist explanation of any kind for so complex a process as imperialism.

Finally, a number of essays address the difficult issue of whether the consequences of imperialism for Third World economic and political development have been on balance positive or negative. Paul Bairoch, in one of the most interesting contributions to the volume, examines admittedly sketchy evidence going back several centuries and concludes that the economic impact of imperialism on the underdeveloped world has been largely negative, although these negative consequences have not been matched by a correspondingly significant positive contribution to the development of metropolitan areas. On the other hand, Ulrich Menzel notes that North and South Korea, Taiwan, and Manchuria, despite a Japanese colonial administration that was unusually brutal and exploitative, have been relatively successful in achieving economic growth. Once again, the wealth of historical detail offered in the book illustrates the difficulty of drawing definitive conclusions about imperialism that hold for all countries at all times.

If there is a weakness in *Imperialism and After* it is that the editors may have tried to address too many issues in a single volume. Any of the themes mentioned in this review could easily have been the subject of an entire, more manageable, project. Moreover, the individual articles vary considerably in length and intent. A number offer extended and detailed case studies and several advance ambitious theoretical formulations, while others are essentially brief commentaries on other contributions to the volume. The articles are, however, of uniformly high quality, and the editors do make a valiant effort to impose order and cohesiveness on a very ambitious and far-ranging project. The result is an important and distinctive contribution to the literature of both imperial history and the study of contemporary Third World development that deserves serious attention from scholars in both fields.

VINCENT A. MAHLER

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**International Dimensions of Land Reform.** Edited by John D. Montgomery (Boulder, CO: Westview, 1984. ix, 239 p.).

The concentration of Third World farmland in a relatively small number of hands has generally been regarded both as a cause of low agricultural productivity and as a potential source of rural unrest. Consequently, for decades social scientists, international agencies, and U.S. foreign-policy makers have been concerned with the need for land reform. Yet, as John Montgomery's concluding chapter notes, international pressures and support for reform have been episodic. Successful postwar programs in Japan, Taiwan, and South Korea—all sponsored by the United States—were followed by failed efforts in South Vietnam and the Philippines. During the 1960s, agrarian reform was a centerpiece of the Alliance for Progress. But, the region's few far-reaching programs in recent decades—Cuba, Peru, Nicaragua, and the aborted effort in Chile—took place because of domestic political changes, not international pressure or support. Recent conflict in Central America briefly revived U.S. interest in land reform, but results in El Salvador have been mixed and U.S. commitment has been limited.

This volume examines at least three dimensions of land reform: the normative and empirical justifications, the record of reform programs, and the extent to which international actors (U.S. Agency for International Development, the United Nations, etc.) have successfully contributed to Third World efforts. Most scholarship on this topic implicitly assumes that redistribution is desirable on the grounds of greater equity, reduced social conflict and increased agricultural productivity. But Joseph Nye's chapter, "Ethical Dimensions of International Involvement in Land Reform," notes the problems of unintended consequences, local autonomy, and national sovereignty. Thus, while supporting reform, Nye argues that international involvement should be prudent and modest. Montgomery summarizes four major areas of consensus within the volume: (1) the negative effect of concentrated ownership on income distribution and efficient land use; (2) the higher productivity (in terms of crop yield or employment generation) of smallholdings; (3) the importance of peasant participation; and (4) the need to supplement land reform with other agrarian policy reforms.

There are, however, some important areas of disagreement among the volume's authors. Albert Berry maintains that extensive land redistribution programs in Asia and Latin America have, in general, positively affected food production and that "reforms that redistribute from large farmers to small ones or form owners to farmer tenants are virtually guaranteed to improve income distribution." Yet John Powelson cites evidence of the negative effects of reform on production. William Thiesenhusen's examination of Latin America concludes that land reform has usually had inegalitarian results, benefiting the upper poor (tenant farmers or full-time plantation workers) at the expense of the lower poor (the landless or part-time farm workers). More pursuasively, Thiesenhusen notes that land reform is rarely supported by the broader package of government policies (affecting credit, internal terms of trade, and import-export subsidies) which would be needed to bring about more equitable income redistribution. Indeed, while several of the volume's contributors have worked with international agencies involved in land reform, in general the book offers a surprisingly pessimistic view of such agencies' records in affecting positive change.

Perhaps the greatest omission in the book is the absence of any significant discussion of Asia. Conversely, the longest chapter in the book is an overly detailed case study of Ethiopia. The chapters vary in quality, with stronger contributions from Thiesenhusen, Montgomery, and, especially, Berry and a disjointed chapter (on international public and private agencies) by Powelson. In all, the book draws upon secondary sources and offers no great theoretical breakthroughs or major new insights. It is, however, a useful examination of many important issues relating to land reform. As such, it is very appropriate for classroom use. While its insistence on the need for broader programs to support land reform and the critical role of peasant input are not new, they should be read and memorized by policy makers and international-aid agencies.

HOWARD HANDELMAN

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**The Soviet Impact in Africa.** Edited by R. Craig Nation and Mark V. Kauppi (Lexington, MA: Lexington Books, 1984. xi, 275 p. $29.00).

If this book's title is seen as a question, the book's answer is little, tentative, partly accidental, and potentially reversible. Although the Soviet Union has a limited presence in a few African countries, notably Ethiopia and Angola, and some special influence in several others, it has not acquired in Africa anything truly significant, such as sister systems or a greatly improved power position in world politics. Nor can it do much more for itself in and through Africa than it has already done, this collection tells us. Even what it has achieved there is partly due to luck or, in more scholarly language, a unique concatenation of circumstances. These included, notably, the collapse of Portugal's empire and the overthrow of the "Lion of Judah" at a time when U.S. foreign policy was being limited by the twin constraints of Vietnam and Watergate. No one of the 10 contributors says or implies that Soviet policy in Africa is either part of some grand plan of expansion or one that has produced great benefits for the Soviet Union. Certainly there is no Soviet "Afrika Korps" engaged in a wave of African conquests or any solid and lasting Soviet political gain in Africa. All is built, the implication is, on sand. African elites have their own agendas, even when they state them in Marxist language. More than one African leader is quoted as saying, "We have not thrown off one group of foreign rulers for another."

The contributors are a multinational set—British, West German and U.S. The focus is primarily on sub-Saharan Africa, though there is one article on the Horn. R. Craig Nation notes in his introduction that the Soviets have had losses as well as gains in Africa and lists some common themes of the contributions, for instance, that debate on the Soviet role in Africa should be moved "outside the confines of a narrow East/West dichotomy," that the interests of the superpowers in Africa "overlap as often as they diverge," and that African instability is due more to "brutal impoverishment" than to any "Soviet meddling." In his other contribution, Nation goes over Soviet motives, means, and prospects and concludes that Soviet interests in Africa are "seldom truly vital" and that much in Africa has "little to do with the USSR." Colin Legum presents the "setting" and a brief history of Soviet-African relations and notes that it is characterized by "erratic and relatively short-lived relationships" with the production of anti-Soviet states out of former clients.

Christopher Coker examines the Soviet and East European role in Africa. (It is good to see the dismissal here and elsewhere in the volume of the old bogey of Soviet interruption of shipping around the cape.) Bernard von Plate concludes from his study of East German behavior in Africa that the German Democratic Republic is much more cautious regarding systemic change there than it was previously and that it now sees socialism more as a possibility for Africa than as an existing process.

Seth Singleton focuses on southern Africa, a place of potentially high opportunity for the USSR, and finds that South African policy actually helps the Soviets. A peaceful southern Africa would have little need for them. Joachim Krause finds that Soviet arms transfers are part of a "dangerous game" intended to give the USSR an African political role and accordingly tend to block solutions and even prevent real Soviet gains. Richard B. Remnek concludes that the unimpressive Soviet military presence in and around Africa has to do more with state interests than with military purposes and is probably not usable in a general war. Marina Ottaway looks at superpower competition in the Horn to find that U.S. and Soviet military power cannot be applied in certain marginal situations and that Third World countries can be adept at manipulating the superpowers. Keith Somerville, in his article on Soviet policy toward Zimbabwe, brings in the important role of the Sino-Soviet dispute and the effects of Soviet rigidity and the backing of losing factions (here, however, the Soviets may now be gaining diplomatically due to recent U.S. policy). The final piece by Mark V. Kauppi is an excellent example of realistic and clear political analysis, pointing out that about half of the reasons for Soviet involvement in Africa are not connected with any definite Soviet "master plan," that no African state is firmly allied with the Soviet Union, and that any Soviet gains push neighboring states closer to the West.

This volume, which also contains several maps and tables, a useful list of abbreviations, suggested readings, and an index would be valuable to those new to Africa and Soviet policy there and would also be of use to specialists. It has a knowledgeable realism and an intelligent and balanced perspective.

KARL W. RYAVEC

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**Chinese Foreign Policy: Developments after Mao.** By Robert G. Sutter (New York: Praeger, 1986. ix, 240 p. $36.95).

*Chinese Foreign Policy: Developments after Mao* is one of the few books on the foreign policy of the People's Republic of China, other than edited works, that focuses exclusively on the post-Mao period. Most of this book, in fact, covers the period 1978 to the end of 1984.

The central focus of the book is the set of common interests between China and the United States. In this context Sutter examines what he calls five "episodes of change" during the six-year period from the time Deng Xiaoping assumed power in China to the end of 1984. The common-interests thread or theme may be considered an assumption, or even wishful thinking, by some critics, the author ostensibly having a personal interest in good U.S.-China relations. Nevertheless this is not a serious impediment to Sutter's objectivity or scholarship.

*Chinese Foreign Policy* is an outstanding work in several respects. The author is thorough. He looks at both domestic inputs, including security issues, and the international environment in assessing Chinese foreign-policy decision making. The imperative of economic development in China as a factor in its foreign policy is analyzed especially well.

On the downside, some readers will think that the author makes too much of cause and effect relationships based simply on time sequence, and others will feel the author is too descriptive and relies too much upon official policy statements, often taking them at face value. This reviewer would concur, but doesn't believe these are necessarily defects in the book.

Perceptive readers will also disagree with some of the author's conclusions. For example, Sutter doesn't think the Sino-Soviet fighting in the Spring of 1969 changed Chinese foreign policy very much. He argues that had already happened, or came later. He likewise does not see it as bringing the U.S. and China together.

Regarding another important issue, the "independent"—some scholars say "equidistant" (from the U.S. and the Soviet Union)—policy adopted by Peking in 1982, the author cites a number of reasons for Peking's decision. Most important are China's national economic interest, relations with the U.S. and Sino-Soviet relations. Sutter does not give much consideration to what some would consider the obvious: that China wanted to play the swing role (between Washington and Moscow) in international, or at least regional, politics in order to play its "deserved" role as a great power.

An interesting point that deserves special mention is Sutter's contention that U.S.-China relations to some extent deteriorated in the early 1980s because Washington did what China wanted to do vis-a-vis the Soviet Union, namely, rebuilt its strategic forces. This made China's "united-front" strategy against the Kremlin less relevant. Probably few readers would disagree with Sutter, though few have heard this argument, because the Western media have seldom if ever mentioned it.

Unlike many books that contain weaker

analysis, the closer the author gets to the present, the better *Chinese Foreign Policy* appears. Sutter's assessment of China's foreign-policy decision making during 1983 and 1984 is especially good. And the best part is on U.S.-China relations. Sutter notes that ties improved during this period because of the succession issue in China (caused by Deng's age), the need for U.S. technology and, ironically, a new Secretary of State (George Shultz) in the U.S. who gave China less weight in dealing with U.S. security problems in Asia.

Sutter concludes that if security and economic issues continue to influence Chinese leaders more than nationalism and ideology, relations with the U.S. won't deteriorate (though China will probably also try to avoid serious problems with the Soviet Union). In short, the author is cautiously optimistic.

*Chinese Foreign Policy: Development After Mao* is unique for its attention to detail. The author understands the subtleties of foreign-policy making in a country that does not tell us much about how decisions are made. He weighs fairly and accurately a variety of foreign-policy inputs. Though Sutter overweighs the economic variables in assessing China's decision making and give inordinate weight to the U.S. factor, in the opinion of this reviewer this is still the best book published recently on the subject.

JOHN F. COPPER

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**Food, the State, and International Political Economy: Dilemmas of Developing Countries.** Edited by F. LaMond Tullis and W. Ladd Hollist (Lincoln: University of Nebraska Press, 1986. xxiii, 351 p. $29.95).

Despite conditions of abundance in today's international commercial-food markets, access to food remains a health-threatening problem for hundreds of millions of citizens in the developing world. At what level of analysis should political scientists try to understand this tragedy? Is it primarily a function of unit-level social circumstances and state policies within the developing world or does it grow out of some larger system-level dynamic? What makes this volume especially useful is that it not only provides an abundance of careful descriptive detail on the international political economy of food and agriculture; it also manages to put forward an abundance of evidence and argument on both sides of this most important theoretical question.

Unit-level explanations for food problems in poor countries are widely accepted by development planners and by most agricultural economists, but they are not yet fashionable among students of international political economy. The editors of this volume therefore deserve credit for offering three superb unit-level studies (on Peru, by Cynthia McClintock; on South Korea by Mick Moore; and on the nations of sub-Saharan Africa, by Cheryl Christensen and Lawrence Witucki) in the first section of the book. McClintock's analysis of Peru nicely contrasts the rhetoric of that nation's food policies to the reality, which has favored neither staple-food production nor agricultural exports, but instead the narrow consumption needs of Peru's own urban middle class. Moore's assessment of South Korea, which is perhaps too gloomy in view of that nation's relative agricultural success, details the political origins and consequences of Korea's recent effort to narrow urban-rural income gaps. The Christensen and Witucki chapter deserves highest praise, as the best single summary analysis of Africa's recent food policies and food problems anywhere in print. It provides all of the arguments and all of the evidence that the reader will need to begin forming opinions on the relative importance of not only unit-level and system-level food policies in Africa but also unit-level and system-level macroeconomic policies.

The importance of working simultaneously at several levels of analysis is also nicely explained in the opening chapter of this section, in a clarifying essay on the meaning of *food security* authored by Raymond Hopkins.

The second section of the book, which takes a system-level look at the "international political economy of food," is in some ways less convincing. Steven Sanderson provides a fact-filled analysis of the internationalization of Latin American cattle production, but he tries to attribute all of the highly unattractive features of this agricultural activity to what he calls "the global logic of capital," as though unit-level factors, such as insecure peasant access to the farm and forest lands being taken over by local cattle ranchers within Latin states, had nothing to do with it. Vincent Mahler's treatment of the evolution of international sugar markets is more descriptive and less doctrinaire. In an interesting twist, it shows that these markets were consciously organized at the system level in colonial times, but are now being shaped more as a result of domestic sugar price-support policies within the U.S. and the European Community. John Bailey's chapter in this section, on agricultural policies in the Western Hemisphere, ends up taking more of a country-by-country, than a system-level, approach. Much the same is true for the final essay by LaMond Tullis on food aid and political instability.

The concluding section of this volume, with essays on technology and food by David W. McClintock and Fernando Homem de Melo, is of some substantive interest but speaks less directly to issues of political-science theory. A final plus to this book is the inclusion of a bibliography of all works cited, as well as a complete subject and author index.

As edited volumes go, this one is the best on the international political economy of food and agriculture in many years, since the publication of Raymond Hopkins and Donald Puchala's *The Global Political Economy of Food* (Madison: University of Wisconsin Press, 1978).

ROBERT PAARLBERG

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## POLITICAL ECONOMY

**Transnational Corporations versus the State: The Political Economy of the Mexican Auto Industry.** By Douglas C. Bennett and Kenneth E. Sharpe (Princeton: Princeton University Press, 1985. xiii, 300 p. $42.00, cloth; $9.95, paper).

This analysis of bargaining relations between the Mexican state and transnational automobile corporations is a major contribution to the study of dependent industrialization in developing countries. The historical-structural approach that the authors employ in their examination of public-policy decision making in a single industry delineates well the multiple, often conflictual relationships that characterize interactions among developing country governments in their efforts to promote national economic development, transnational corporations (TNCs) competing for profits and market share on a global scale, and national entrepreneurs seeking to establish a presence in an important new industry. The result is "a case study of the limits of possibility for a state seeking to *overcome dependency* while abiding by the norms of global capitalism" (p. 9).

Bennett and Sharpe develop a framework for analyzing state-TNC bargaining that focuses on agenda setting, decision making, and policy implementation. They use this framework in their examination of three bargaining episodes (in 1962, 1969, and 1977) in which the Mexican state sought to determine the market and ownership structure (the number of firms in the industry and the extent of domination by transnational firms) and performance characteristics (especially export promotion) of an emerging automobile-manufacturing industry. The authors are particularly concerned with evaluating the different factors that shaped the interests, power, and bargaining capabilities of the Mexican state and automobile TNCs under changing national and international circumstances, in an attempt to determine "the possibilities for change within and against existing structures" (p. 10). Their reference to industrial-organization theory to explicate TNC behavior and the sources of TNC bargaining power is especially useful in this regard. In addition, the analysis of several different bargaining encounters spread over two decades (including a brief postscript describing a 1983 Mexican-government decree regarding the automobile industry) underlines both the dynamic character of state-TNC interactions and the extent to which the Mexican state learned to improve its bargaining capabilities over time.

The authors also analyze the outcomes of these state-TNC bargaining encounters, including the ways in which the state's initial failure to prevail on key issues permitted subsequent TNC activities that caused or aggravated distortions in Mexico's economy and society. These problems included the progressive denationalization of the terminal (vehicle manufacturing) industry as TNCs used access to capital, technology, and international marketing networks to expand their presence in the Mexican market at the expense of state-owned enterprises and those firms with some degree of private local capital; the development of an inefficient, high-cost national automobile industry that depended on a restricted, high-income, domestic consumer market and lacked the ability to compete at the international level; and the persistence of an import-dependent automobile sector that constituted a long-term drain on the country's balance of payments. The discussion of developments in the automobile parts-supply industry (where the state consistently enforced restrictive legislation limiting the extent of transnational firms' participation in parts manufacturing and facilitating the expansion of private Mexican enterprises) supports Bennett and Sharpe's conclusion that alternative state policies might have avoided or attenuated some of these difficulties.

Although Bennett and Sharpe's conceptualization of the state and state interests is a useful contribution to debate on these questions, their analysis of the historical development of the postrevolutionary Mexican state is less successful. The authors ascribe considerable freedom of action to the emerging postrevolutionary governing elite in their discussion of "the decision to reintegrate Mexico into the world capitalist economy" (p. 21) following the 1910-17 revolution. Yet some recent scholarship seriously questions the extent to which the revolution disrupted domestic class structures and Mexico's economic dependence on foreign (particularly U.S.) capital, thus suggesting major constraints on the state's relative autonomy during this period. Much of the book's discussion concerning initial state interests emphasizes the capitalist preferences of ruling political and economic groups based in northwestern Mexico. The influence of this "Sonoran dynasty" was certainly important, but Bennett and Sharpe make little reference to the possible ways in which the character of the revolutionary experience and growing peasant and labor demands may have affected the evolution of postrevolutionary development strategies. Moreover, the authors imply that state orientations established in the early postrevolutionary period—including a preference for private-sector economic initiatives and the "last-resort" character of state actions to promote development in key sectors—were largely unaffected by the class conflict, mobilization of mass actors, and clash of rival development strategies that dominated much of Mexican politics from the 1930s to the early 1950s. These problems suggest the larger need to address more thoroughly the changing national political context, the postrevolutionary regime's mass (labor and peasant) bases of political legitimacy, and the impact of mass actors on the evolution of national-development strategies.

Despite difficulties such as these, Douglas C. Bennett and Kenneth E. Sharpe have written a significant book that is on the whole theoretically stimulating, carefully researched, and very well written.

KEVIN J. MIDDLEBROOK

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**The Economics of Zoning Laws: A Property Rights Approach to American Land Use Controls.** By William A. Fischel (Baltimore: Johns Hopkins University Press, 1985. 372 p. $35.00).

Local governments in the United States have a wide array of zoning instruments at their disposal, and they do not hesitate to use them. Over the years, the range of available instruments has increased as new methods of control have been invented and accepted by the courts. In residential areas, controls on height and bulk of buildings have been supplemented by restrictions on lot size, floor space, families per dwelling, and other dwelling characteristics. More recently, density controls and direct controls on population growth have been introduced. The use of these instruments is the subject of much controversy and confusion. The subject is an important one and has gained increasing prominence as state courts (led by

New Jersey) have taken an increasingly active role in overseeing and directing the exercise of local zoning powers.

In *The Economics of Zoning Laws: A Property Rights Approach to American Land Use Controls,* Fischel undertakes to provide a systematic analysis of zoning, using economic analysis and an emphasis on the role of property rights. The analysis is conducted with a healthy regard for the importance of institutional arrangements, legal doctrine, and political processes.

The early chapters of the book detail the evolution of zoning laws. The constitutional foundations of challenges to zoning ordinances are outlined, and key court cases defining the legal status of zoning are reviewed. The roles of various actors in administering (and challenging) zoning laws are also detailed.

The heart of the book is the development and application of an analytical framework for evaluating land-use controls. The Coase Theorem—the claim that resource allocation will not depend on the initial distribution of entitlements—is carefully developed. Key assumptions underlying the theorem are investigated in considerable detail. The importance of a complete definition of *entitlements* (property rights) and conditions permitting for costless protection and transfer of those entitlements are emphasized. Fischel also nicely contrasts the emphasis of the property-rights approach with that of the externality approach.

Fischel identifies zoning of undeveloped suburban land as the most controversial application of land-use controls, and a significant part of the book addresses this important problem. He proposes a criterion for limiting the use of zoning: it should be used for providing local public goods but not for redistribution. In what may be the most controversial part of the book, he proposes a method for establishing initial entitlements grounded in a definition of "normal behavior."

The politics of land-use controls receive attention throughout the book. The protagonists in land-use disputes are identified and their influence on outcomes in various arenas (rural, suburban, central-city) is discussed. While no formal model of political decision making is developed, the median voter model is frequently invoked to suggest the relative power of combatants in a particular setting.

This book is valuable not only for the systematic analysis of land-use controls that Fischel provides, but also for the good sense and perspective that he brings to the issues. Some examples: He argues persuasively that exercise of monopoly power is not a major reason for restrictive zoning by suburban communities, and he marshals convincing evidence to support his claims. He effectively debunks the notion that urbanization is using up "too much" farmland, and again presents evidence to make the case. He argues cogently that "fair-share" rules are likely to do more harm than good in promoting low-cost housing for the poor.

Readers who are not professional economists will find Fischel's presentation easily accessible. He relies on verbal arguments and simple diagrams, using little jargon. His style is lively and readable. The book is an excellent introduction to the subject of land-use controls. Those well-acquainted with the subject, too, will find Fischel's analysis interesting and thought-provoking.

DENNIS EPPLE

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**Read the Label: Reducing Risk by Providing Information.** By Susan G. Hadden (Boulder, CO: Westview, 1986. xviii, 275 p. $27.50).

**The Language of Risk: Conflicting Perspectives on Occupational Health.** Edited by Dorothy Nelkin (Beverly Hills: Sage, 1985. 200 p. $25.00, cloth; $12.50, paper).

Government policy over the last quarter century has featured an increased awareness of health and environmental risks. In varying degrees, the Hadden and Nelkin volumes advance understanding of the subject. The former volume analyzes the uses of labels and other forms of information as a policy strategy for reducing health risks. The latter explores the conflicts and language of risk that characterize discussions of occupational health.

The title of Hadden's volume prompted me to brace for a procrustean attempt to prove that markets and information provide a panacea for coping with risk. In fact, the book is a remarkably balanced, sophisticated, and powerful analysis of labeling policies—their limits and potential. Hadden understands the appeal of information provision as a strategy. It is a relatively mild form of regulation which emphasizes individual choice and liberty. The need for labeling policy arises because markets tend to underproduce accurate, comprehensible hazard information.

Early in the work, Hadden develops a set of propositions based on microeconomic theory as to when regulation by information seems more suitable (e.g., when risks and benefits tend to be borne by the same individual and are short term). She then devotes five chapters to analyzing the applicability of the propositions with respect to drugs, consumer products, occupational risk, food, and pesticides, respectively. She finds that policy and practice often depart from what these propositions would make one expect.

While Hadden's work achieves great depth, originality, and plausibility throughout, four themes deserve particular note. First, she underscores the limits to rationality when people respond to information about risk. In her view, "the evidence that people do not understand risk information very well is nothing less than overwhelming" (p. 215). Inappropriately designed labels contribute to this substantial problem. Second, she stresses the regressive quality of regulation via information. People of greater education and income are the most adroit at responding to risk information printed on labels. Hadden cogently explains why "a policy that maximizes individual freedom to exercise risk preferences in effect forces the poor to be risk-prone" (p. 226). Third, Hadden reveals a rare ability to mesh macro- and microlevel policy concerns. Persuaded of the need to strike an appropriate balance between direct regulation and information provision, she offers specific suggestions for improving labels. Finally, Hadden firmly grasps that dealing with risks via information entails increased government intervention on behalf of this objective. Needed reforms range from improving labels to upgrading the quality of schools.

Nelkin's anthology aims "at clarifying the diverse and often conflicting modes of thought that enter the discourse and affect public policies concerning occupational health" (p. 13). Three of six essays in the volume explicitly probe the language of debate in this policy sphere. Stephen Hilgartner examines ways in which industry and labor advocates present their views about occupational health. He emphasizes the uses of language as a political tool. Chris Anne Raymond adopts a similar approach to describe how the mainstream and advocacy press deal with occupational health issues. She portrays news as ideology. Neither the Hilgartner nor the Raymond essay generates strikingly new propositions; but both succeed in vividly illustrating conflicting rhetorical styles and intellectual underpinnings.

In a more provocative vein, Mark Sagoff focuses on the language of analysis. He contrasts the engaged perspective of activists with the detached perspective of economists. Sagoff argues for the importance of both perspectives. However, he contends that the fundamental reason for regulation is not to make markets efficient or to maximize benefits over costs (the detached perspective) but to minimize sickness, injury, and death (the engaged perspective). In his view, the detached perspective proves most useful in discussing cases where hazards are small, and the costs of reducing them great. Sagoff sees the detached perspective as providing rhetorical justification for policies we demand on the basis of engaged ethical sentiment.

The three other essays in the volume do not so explicitly focus on the language of occupational health. Dorothy Nelkin capably dissects the conflicting role pressures on the corporate physician. She raises serious questions as to whether these physicians respond to the health needs of workers. For his part, Michael S. Brown contributes the best essay in the book, one focused on worker access to and use of hazard information. He presents six case studies focused on what happens when workers try to inform themselves about hazards and act on this information. Brown concludes that in the adversarial context of occupational-health policy, regulation via information may do little to protect most workers. Those represented by unions stand the best chance of benefitting from such measures.

Sheila Jasanoff's essay focuses on the "misrule of law" at the Occupational Safety and Health Administration (OSHA). She contends that the litigation model for framing and resolving regulatory dilemmas has failed at OSHA. She may be correct, but her essay does not systematically develop the point. Jasanoff contends that the adversarial processes of litigation have led to the triumph of "political considerations" over "good science" and contributed to a situation where "ideological motivations have continued to take precedence over managerial considerations" at OSHA (p. 169). However, Jasanoff does not show how alteration or abolition of the litigation model would enhance OSHA decision making. The promulgation of health standards poses tradeoffs. Perceptions of these tradeoffs currently fuel the fierce conflicting pressures on OSHA from unions and businesses. Jasanoff does not demonstrate how procedural reform would enhance the way OSHA handles these tradeoffs and pressures—why it would cause politics to "improve."

Both the Hadden and Nelkin books deserve the attention of students of regulatory policy and risk. What should scholars do if they lack time to peruse both volumes? Read Hadden.

FRANK J. THOMPSON

*University of Georgia*

**Coal Firms under the New Social Regulation.** By Richard A. Harris (Durham, NC: Duke University Press, 1985. x, 188 p. $30.00).

**U.S. Coal Abroad: A Social Action Perspective on Interorganizational Networks.** By Kathryn S. Rogers (Westport, CT: Greenwood, 1986. xii, 259 p. $38.95).

Coal is the United States' most abundant fossil-fuel resource, an essential, controversial component to any analysis of national energy policy or economics, yet almost ignored by political science. The two studies by Richard Harris and Kathryn Rogers, both concerned with the response of U.S. coal firms to rapid change in their political and economic environment, thus provide a wealth of welcome information about important recent changes in the political and economic behavior of coal firms framed in terms of substantial theoretical concern to political scientists.

Richard Harris's clear, concise study seeks to understand how business firms are responding to the new "social regulation" by examining the impact upon U.S. coal firms of the Surface Mining Control and Reclamation Act (SMCRA) of 1977. After reviewing relevant political and economic theories of regulation, Harris asserts that the behavior of these firms challenges several conventional assumptions: (1) firms share a community of interest and respond collectively to regulation; (2) trade associations and other collective representatives are the primary actors in the politics of regulation; and (2) firms have the same interest and priorities in early and later stages of regulation. In Chapter 2, the author proposes several new variables for the traditional microeconomic model of firm behavior under regulation because the "subgovernment setting of regulatory policy places the simple microeconomic model of firm behavior in a more complex political-economic context. Thus, the individual firm assesses regulatory policy in terms of its impact on the profit function, forms preferences based on that assessment and selects its behavioral response subject to the subgovernmental constraints on its probability of influencing regulatory policy" (p. 25). Chapter 3 then describes SMCRA's new regulatory subgovernment.

The remainder of the book examines how well this revised model accounts for coal-firm behavior under SMCRA. Chapter 4 describes how size and region create a plurality of political and economic interests among coal firms. Chapters 5 and 6 describe how these differences create different political strategies and priorities among coal firms. Chapter 7 attempts to summarize the impact of "new regulation" on the coal industry, and chapter 8 concerns the impact of the Reagan administration's deregulation on the coal industry. Harris concludes that new regulation has brought about "a fundamental change in the political system" (p. 151). It has politicized the individual firm and diminished the importance of trade associations in regulatory politics. It has also given differential advantage to regulated firms in the same industry. "If SMCRA typifies regulation under the new regime," Harris asserts, "influential firms will be new, relatively capital-intensive [and] large and will view as legitimate the new regulatory goals" (p. 148).

This is a readable, substantial contribution to the theoretical and empirical literature on regulatory politics, political economy, and interest-group behavior. There are also some deficiencies. Generalizations about the impact of "new regulation" based on limited experience with a single industry are always tenuous.

The author rather blandly assumes that profit functions of firms similarly sized and located are identical without any direct evidence or consideration of the special case of interregional and "captive" firms. The complexity of motivations and interests promoting SMCRA is reduced to an "environmentalist" conspiracy and readers are gratuitously lectured on environmentalism's economic shortcomings. Nonetheless, the book deserves a wide audience and should achieve it. In addition to the major conceptual issues it raises, it provides essential information about the history and politics of SMCRA. Lucidly organized and free of jargon, it offers something to both scholars and lay readers.

Kathryn Roger's study of the coal industry's response to new export markets for steam coal is considerably more technical and narrowly defined in its audience. The author argues that "traditional explanations of the relationship between . . . industry and the environment are inadequate to explain inter-organizational patterns and changes in inter-industry power" (p. 7). She believes that a "series of simultaneously overlaid patterns of inter-organizational relationships," rather than a single pattern of adaptation, characterized the coal industry's reaction to the new export market between 1980 and 1982. The author then identifies and describes five specific response patterns: (1) competition for sales, (2) organization and use of new information sources, (3) transindustry and intraindustry coalitions, (4) supraorganizational activities, and (5) "macro-cooperation in policymaking." Using a conceptual model derived from "social-action" theories in organizational sociology (explained in chapter 2), the author explores in succeeding chapters how each of the five organizational responses occurs, and why. A final, very brief chapter attempts to relate the findings to other theories of organizational behavior.

The study will appeal, as the author expects, mostly to management scientists, institutional sociologists and policy makers concerned with business-government relations. The enormous wealth of information and detail adds considerably to the literature on coal-firm behavior in an international context and raises some important issues for organizational theorists. This is essentially a doctoral dissertation with a few cosmetic touches, and therein lie some problems. The book begs for sound editing. The prose is often excessively technical and arcane, bristling with jargon. Several of the diagrams like the "Pictorial Model of the Coal Industry's Network" (p. 15) resemble a Rorschach Test. Ideas and their nuances are frequently explored in excruciating detail. But there are valuable ideas and insights if one is willing to persevere.

WALTER A. ROSENBAUM

*University of Florida*

**The Sustaining Hand: Community Leadership and Corporate Power.** By Bryan D. Jones and Lynn W. Bachelor with Carter Wilson (Lawrence: University of Kansas Press, 1986. vii, 247 p. $27.50, cloth; $9.95, paper).

A documentary film about the eminent domain seizure of Detroit's "Poletown" neighborhood for the construction of a huge General Motors Cadillac plant has been making its way around university classrooms for the past several years. The climax of this video polemic occurs when a large wrecking ball smashes into the cross-topped steeple of Immaculate Conception Church, while aged Polish-American parishioners look on in tears. Most of the written accounts of this chapter in Detroit's economic-development history similarly suggest that Poletown was brutally ravaged by unfeeling public authorities to serve the dubious private ends of a rich multinational corporation. In contrast, the presentation of the tale of this episode by Bryan Jones and Lynn Bachelor offers a much needed and nicely balanced counterpoint.

The case study of Poletown, bolstered by two additional and much shorter accounts of analogous efforts by the cities of Flint and Pontiac to keep General Motors facilities from leaving their communities, is a vehicle used by the authors to explore the relationship between political leadership and economic power. Two major factors converge to constrain and shape this relationship, and it is at this intersection that the politics of economic development are typically played out. On the one hand is the high political priority that local leaders have come recently to accord to their constituents' employment needs and the leaders' sense of responsibility to foster these through various economic-development initiatives. On the other hand, there is the relative ease with which modern capital can change location in quest of more profitable or efficient sites. This was an especially important consideration in these Michigan cases because the automobile industry was in the process, in the late 1970s, of replacing and retooling its production facilities to meet the import competition and adapt to the oil crisis that eroded industry profits during that decade. On the face of it, the combination of political incentives and corporate mobility would indicate that economic elites could easily command sufficient leverage to control public decision making in communities hungry for investment.

But this book is devoted to the proposition that while corporate power is immense, political leaders are not helpless in the face of it. General Motors did constrain the choices open to the mayors of the three Michigan cities but still left enough leeway, often inadvertently, for them to influence and shape the final outcomes of negotiations to their cities' advantage. Detroit's Coleman Young in particular seized the opportunity to find an appropriate site for a new General Motors plant in the city, even though the corporation's offer to let Detroit bid on the facility was apparently made in the genuine expectation that neither the stringent conditions nor the tight timetable could be met.

But the city acted swiftly and imaginatively to put together an elaborate financing package with a combination of federal grants and tax-increment financing (TIF), offered a cleared site (necessitating the acquisition of 17 hundred pieces of property and the relocation of nearly 35 hundred people) in a racially mixed area that straddled the border with Hamtramck, MI. General Motors, concerned in part about the public-relations consequences of pulling out of Detroit and presented with virtually all that it had sought in the way of location requirements, accepted the offer to build its new facility in the city, thus saving several thousand jobs and promising to add tax revenues to the city's treasury at some distant time when the tax abatement and TIF arrangements expired.

In the process the city provided extremely generous settlements to homeowners who were displaced. Though neighborhood opposition was vocal, it was, by this account, limited to a small number of people. General Motors even made an offer to move the Immaculate Conception Church, even though officials of the archdiocese had already sold the building to the city.

The authors do not present a Pollyannaish tale. They are aware of the human costs of big economic-development projects and of many of the ironies involved in the use of public power for private corporate advantage (for example, financing cost overruns in Poletown meant that projected tax revenues from the area would be less than the cost of servicing the debt). But their case study provides a convincing argument that such efforts may constitute entirely justifiable responses to the present reality of municipal dependence on private investment for jobs and tax resources.

PETER EISINGER

*University of Wisconsin*

**Time, Chance, and Organizations: Natural Selection in a Perilous Environment.** By Herbert Kaufman (Chatham, NJ: Chatham House, 1985. xii, 180 p. $20.00, cloth; $11.95, paper).

This provocative work introduces readers to the evolutionary analysis of organizations, a mode of theorizing more commonly found in sociology. The theory's basic premises are simple: (1) organizations find it hard to change their fundamental structures and processes, and (2) turbulent environments episodically kill off established organizations and create new ones. Together, these assumptions suggest that most organizational change derives from changes in the *composition* of organizational populations rather than from adaptive changes of individual entities.

This idea has significant implications for the study of burueaucracy. It suggests, for example, that examining birth and death rates of agencies, as Kaufman did in his empirical monograph, *Are Government Agencies Immortal?* (Washington: Brookings Institution, 1976), will provide information about an organizational population's rate of collective adaptation and that examining the date of an organization's founding will provide information about its fundamental structural properties, as Arthur Stinchcombe described in his classic essay, "Social Structure and Organizations," in the *Handbook of Organizations* edited by James G. March (Chicago: Rand McNally, 1965).

The book is well organized around this basic argument. Kaufman prefaces each chapter with a question and a one line answer, and each chapter elaborates that terse answer. The book is concise and lively and contains a helpful guide to the relevant literature.

It is not, however, merely a summary of other evolutionary arguments; Kaufman has a distinct perspective. First, he takes a rather hard line on the significance of adaptation: change at the level of the individual organization is held to be unimportant. Though this view has been put forward elsewhere, notably by Michael Hannan and John Freeman, it is not a necessary component of evolutionary analyses: for example, Richard Nelson and Sidney Winter contend in their *An Evolutionary Theory of Economic Change* (Cambridge: Harvard University Press, 1982) that both adaptation and populational change matter.

Second, Kaufman takes a novel and controversial position on organizational demise. Most evolutionary analyses accept the proposition that much variation is "blind"; that is, it is not designed for the type of environment in which an entity finds itself. (The blind randomness of genetic variation is one of the cornerstones of the neo-Darwinian theory of biological evolution.) Selection, however, is usually regarded as a function of fitness—how well adapted an organism or organization is to its environment. Here Kaufman goes out on a limb by maintaining that the *selection* of organizations is basically random as well. This is a startling claim, but Kaufman makes no bones about it: "chance must be the mechanism of selection" (p. 70).

The idea of random demise might be useful as a null hypothesis, a standard that theories of systematic selection must beat. Such baseline models have proven useful in other parts of organization theory: for example, in the study of the size distribution of firms, Ijiri and Simon's Markov model of random proportional growth has done quite well. But Kaufman apparently does not interpret random demise as a null hypothesis; it is a primary object of interest. This emphasis may indicate a misinterpretation of the role of luck in selection. Most evolutionary theories admit chance in selection in the sense that it is usually assumed that fitness does not ensure survival; instead, a more fit entity has a higher *probability* of surviving. Hence it is possible, though unlikely, for a less fit entity to outlast a fitter one. This is intuitively plausible: the fit are not always lucky, but in the proverbial long run even small differences in fitness can matter if the selection environment is stable.

By the same token, of course, small *adaptive* changes can also accumulate to large differences in organizational performance, as Lindblom, Wildavsky, and other incrementalists have argued. For this reason the two aforementioned assumptions of the evolutionary approach do not rigorously entail that most organizational change is populational; they only suggest it might be so. The issue is fundamentally an empirical one, involving quantitative comparisons of the rates of populational versus adaptive change. Ironically, Kaufman's earlier empirical study does not unequivocally support his conjecture that populational turnover is the primary source of bureaucratic change. His data indicate that agency death rates are slower than those of firms in general and far slower than that of the organizational drosophila (i.e., restaurants) examined by Hannan and Freeman. Though Kaufman's measurements are only one half of the needed comparison, they are not a promising empirical beginning for his conjecture.

Unfortunately, the evidence in this area will inevitably be plagued by a host of measurement problems for the foreseeable future. Hence, it is unlikely to prove decisive. Further, the a priori reasons for believing in the primacy of either turnover or adaptation are not compelling. Therefore, on both empirical and theoretical grounds a prudent strategy would be to follow Nelson and Winter's approach of modeling both populational changes (selection) and adaptive behavior (search). Their work not only demonstrates that one can create internally consistent models representing both types of change but their simulations systematically explore the implications of varying the relative importance of selection versus search.

I believe that the study of organizational change will progress more rapidly if specialists follow Nelson and Winter's lead rather than Kaufman's, but *Time, Chance, and Organiza-*

*tions* is certainly an enjoyable introduction to a mode of evolutionary reasoning.

JONATHAN BENDOR

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**Collective Decision Making: An Economic Outlook.** By Shmuel Nitzan and Jacob Paroush (New York: Cambridge University Press, 1985. x, 132 p. $29.95).

While classical political theorists (e.g., Rousseau) often considered collective decision making (e.g., voting) to be a method for determining the "collective will," the will of God, or other collective goods, most recent theories have reduced collective decision making to a negotiation among competing interests. Politics involves both competing interests that must be reconciled *and* the selection of alternatives that improve the overall welfare (however defined). Individuals are not only the repository of preferences that can conflict but are also the repository of information that can be cumulated to their mutual benefits. As an important supplement to the social-choice literature concerning conflicting preferences, this book is part of a revival of interest (initiated by Duncan Black) in the classic but understudied problem of understanding how collective-decision-making mechanisms can be used to make the best decisions.

Nitzan and Paroush have been prolific contributors to the theory of optimal collective decision making. This book provides a very useful and provocative introduction to this field of inquiry through the integrated presentation of their own (mostly previously published) work. The first two chapters provide the context for the rest of the book. Chapter 1 provides a useful nonmathematical overview of the nature of the problems and solutions. Chapter 2 puts forth the major theoretical point of the book, their basic theorem (independently discovered by Shapley and Grofman) concerning an optimal collective-decision rule. They show that the optimal rule for making dichotomous choices (based upon choices made by individuals choosing independently with varying probabilities of being correct) is a weighted majority rule, where individuals are weighted by their competence (specifically, if $p$ is the probability of an individual making a correct choice, the individual's voting weight is $\log [p/1 - p)]$).

The rest of the book is devoted to extending the line of reasoning in a variety of interesting and important directions. Nitzan and Paroush provide simple and useful theorems concerning optimal decision procedures under a variety of conditions including costs of decision making, asymmetry among alternatives (e.g., inertia), incomplete information on competence, interdependence among individuals, the possibility of investment in individual competence, the possibility of sequential decision making, and consideration of multiple alternatives rather than two. The treatment is a combination of text and mathematics. The text generally provides a clear statement of the problem and the nature of the solution; and, in pleasant contrast to common practice, the mathematics is accessible.

The book concludes with a disappointing chapter (chapter 10), "Applications." With such a general framework, I had hoped that many concrete policy implications (choices of decision mechanisms) might be drawn. While the theory itself is tautological, the assumptions (e.g., independence of individual decisions) are unlikely to be perfectly met. Consequently, one might ask where the conclusions (e.g., the optimality of the weighted majority rule) are most useful.

This book provides important insights into the nature of collective decision making as well as the possibilities for optimal use of collective-decision mechanisms. It should become standard reading in courses that have heretofore been confined to considering collective decision making as a mechanism for reconciling conflicting individual interests. I hope that readers are stimulated to develop this approach and to find ways to integrate it with the more common treatments of collective decision making. Ultimately, such integration may make it possible to understand how real collective-decision processes can be simultaneously used to resolve conflicting interests and make collectively beneficial decisions.

SCOTT L. FELD

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**Capitalism, Consumption, and Needs.** By Edmond Preteceille and Jean-Pierre Terrail. Translated by Sarah Matthews (Oxford: Basil Blackwell, 1985. 220 p. $34.95, cloth; $15.95, paper).

The nature of human needs is a vexing issue in political theory. Marx, of course, was convinced that our fundamental material, intellectual, and spiritual needs inevitably go unmet in capitalist society, but he never elaborated a theory of needs. That lack has haunted Marxism ever since.

Edmond Preteceille and Jean-Pierre Terrail attempt to remedy the situation in a book that promises a theoretical analysis of "the very concept of needs" (p. 3). Except for the last chapter, the book was written a decade ago, and only recently—often infelicitously—translated. It has a dual purpose: to contribute to Marxist economics and sociology and to engage in a polemic on the crisis of French capitalism. Thus, some familiarity with Marxist terminology and French intellectual and political life will help the reader.

In chapter 1 Terrail criticizes liberal and other non-Marxist positions for misconstruing need as an "objective lack," something engendered outside economic activity, and as the original impetus behind human action. This "idealism of needs" (p. 10) posits consumption as a process that is distinct from and logically precedes production and drives it in an instrumental fashion. Even the differentialist sociology of Veblen, Bourdieu, Baudrillard and others, which criticizes the simplistic liberal view and accepts a link between consumption and production relations, ends up in an idealist trap by psychologically interpreting the social. Terrail presents the solution in chapter 2, with an account of the social and historical nature of needs. Here we find that historical materialism provides the key by recovering the primary role of the means and relations of production: "the needs that production satisfies are the needs of production itself, the demands of its reproduction" (p. 39). The liberal position is reversed; consumption is actually "driven" by production. And needs, which are neither natural nor fixed, are defined as follows: "the objective demands of the mode of production with regard to its agents" (p. 58); "the result of a process of internalisation of the needs of production" (p. 60); and "subjective forms of the objective determination of the social practices shaping the reproduction of labour-power" (p. 174).

In chapters 3 and 4 Preteceille applies this understanding to an empirical examination of consumption patterns in France during the current "crisis of state monopoly capitalism." He labors to show that individual consumption is being increasingly joined by socialized consumption, especially through state-supported public services and collective facilities. This development illuminates the social and political nature of consumption, stretches the capacity of capitalism to meet needs that it has created, and fuels the movements of self-management and voluntary associations. The authors conclude by criticizing the French Communist party's insensitivity to the diversity of workers' needs and by championing pluralism for assisting in the "development of diversified and democratic expression of needs" and as a positive step toward socialist revolution (pp. 199, 205, 216).

This book is filled with helpful insights and humanistic sentiments, such as the call to "stop confining the expression and satisfaction of needs to the area of consumption" (p. 165). But it is hindered by some aggravating weaknesses. The painful consistency of the language of historical materialism constantly evokes a vision that is lifeless, spiritless, and humorless. As is often the case with "materialist" Marxism, the analysis lapses into a fetishism of the "concrete" (read "specific"). While the authors valiantly try to avoid economic determinism and reductionism, their view of Marxism is nonetheless laden with positivistic overtones, making their discussion often rigid and formulaic. One never gets a real sense of the richness of the question of human needs. Finally, the book is often repetitious.

Fortunately, a dialectical (nonpositivist) Marxism has more to offer on the question of needs than what we find here. The work of Marcuse or Habermas comes to mind immediately. Nonetheless, the topic is crucial, there is too little written about it, and this book should be read.

SCOTT WARREN

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**The Logic of Collective Choice.** By Thomas Schwartz (New York: Columbia University Press, 1986. xiv, 315 p. $35.00).

The topic of this book is axiomatic social-choice theory. It is not a book for the casual reader. It is, however, well worth the time and effort required for its reading.

Beginning with a general model of choice and preference, Schwartz leads us unerringly through a maze of impossibility results and "paradoxes," both known and novel. Throughout, he is careful to elucidate the substantive content of axioms and results through informal discussion, example, and the occasional burst of Socratic dialogue ("*Question:* You take it for granted that choosers are confronted by feasible sets, whence they must choose. But how are these sets determined? . . . *Reply:* I fully agree that identifying the feasible sets is an important, interesting, neglected part of the total choice process. It is the subject of chapter 10. . . . *Question:* You assume choosers invariably choose *some* feasible alternative. . . . Is it not possible to choose *nothing?* . . . *Reply:* Not as I use the verb *to choose*" [p. 12]). The path along which we are led leads, in chapter 5, to the issue of where to lay the blame for social-choice impossibilities. Schwartz makes a four-fold taxonomy of axioms used in the theory: minimum-democracy conditions (e.g., social choice must not be dictatorial), independence conditions (e.g., only individuals' preference rankings over alternatives are relevant for social choice among those alternatives), domain conditions (e.g., social-choice rules must be applicable whatever the list of individuals' preferences happens to be), and collective-rationality conditions (e.g., social choices must be transitive). For Schwartz, the culprit is clearly the last of the four: "Stability is not obviously a virtue in all cases (exactly what is wrong with the continual overturning of choices?). . . . *Cycles* [in social choices] can exist without *cycling*, and cycling is not necessarily a Bad Thing" (p. 127).

The normative crux of the argument for this conclusion is that, unlike the other three classes of condition, the collective-rationality conditions say nothing about how social decisions should depend on individual decisions: "rather [they] are abstract, mathematical theorists' assumptions" (p. 124). Moreover, as ethical requirements on social choice, those axioms from the first three categories listed, which are sufficient to precipitate an impossibility result in the presence of any nonvacuous rationality restriction, "are so utterly weak as such conditions go . . . the blame for any conjoint unreasonableness [in the set of axioms] must lie with Collective [rationality]" (p. 125).

Schwartz is persuasive. Nevertheless, there is, I think, at least one reason why collective rationality conditions amount to more than simply "mathematical theorists' assumptions," and, thus, why his conclusion is not the only reasonable one here. Typically, social-choice theory is perceived as being concerned with the positive analysis of existing methods of collective choice, and with the delineation of feasible classes of such methods. But it is also germane to evaluating government action, to the question of public accountability, and to normative political debate. When, say, the president initiates some new defense program, it is legitimate to inquire why, to require the president to justify his action: "Of all the possible things you could have done in regard to national defense, why did you choose *this* particular alternative?" For the president's response to be acceptable, it must at least be internally consistent and invoke reasons derived from principles other than pure self-interest or political expediency (whether it is these last which in fact motivated his choice, or not). When some individuals lose by the presidential decision, they are entitled to some rationale for *why*, and simply to be told either "*Someone* had to lose, so it might as well be you," or "I spun the White House roulette wheel and this is what turned up," is hardly satisfactory. In other words, a legitimate justification here requires reasons not depending on caprice or chance for making the final, unique, choice from the set of alternatives. It is an internally consistent argument based on general principles of *collective* choice, despite being a decision of a single individual. From this perspective, which is related to Arrow's defense of transitivity *Social Choice and Individual Values*, 2d ed., (New Haven: Yale University Press, 1963, p. 120), rationality conditions are desirable normative constraints on the public arguments of public officials.

These last observations bear on the relevance and substantive interest of interpersonal comparisons and preference intensities in social choice. Schwartz explicitly devotes "very little attention to collective choice processes based on preference intensities" (p. 30), and he does so for two reasons: "First, such processes are thoroughly unrealistic. None is used in the real world," and "second, it is questionable whether collective choices *should* be based on preference-intensity information, even when such information is ready to hand" (p. 30). Apart from demonstrating later (pp. 104-13) that introducing preference intensities and interpersonal comparisons is no panacea for paradox, Schwartz confines his discussion of the issues to supporting the two reasons for ignoring them. Yet invocations of principles of fairness based more or less explicitly on interpersonal comparisons are commonplace in political argument. In my earlier example, the president might, for instance, tell the losers in his decision that they are believed to be able to bear the losses more easily than other potential losers because they are wealthier. Of course, this belief may be mistaken; but in and of itself this does not obviate the importance of such claims for political decision making and legitimacy. Consequently, even if it is the case that no extant collective-choice mechanism uses preference-intensity information, understanding the implications and structure of different forms of fairness criteria that depend upon such information seems both relevant and important.

While the first half of the book addresses essentially normative issues, the second half is clearly positive and certainly the most original and provocative part of the text. Following from his conclusion on the undesirability of collective rationality conditions, Schwartz goes on to study social choice processes in the absence of such prerequisites. He does this through proposing and analyzing various solution concepts for given choice mechanisms. A solution concept is a specification of "the set of possible outcomes that could obtain if participants [in the collective decision-making process] acted rationally, given their preferences" (p. 139). Some of these concepts are familiar from Schwartz's earlier work Generalized Top-Choice Assumption and Generalized Optimal-Choice Axiom (GETCHA and GOCHA) and are defined in terms of the binary preference relation derived from paired choices. Other concepts are new Solution Conditions (SOCOs 1 and 2) and are instead defined in terms of power relations, which, loosely speaking, specify when a group of individuals can unilaterally change a given social "decision" to a particular alternative.

These last in particular are predicated on a game-theoretic view of collective choice. Given this, it would have proved useful to have the underlying structure of the collective-choice game made more explicit than it is. For example, in chapter 7 there is an extensive discussion of the notion of power in collective decision making in terms of outcomes that particular groups can achieve in various circumstances. However, at no point in the discussion are the strategy or information sets available to "players" made explicit. (If we assume complete information and restrict strategy sets to feasible sets of preference relations, then Schwartz's concepts of power seem very closely related to the idea of an effectivity function, pioneered by Bezalel Peleg (see in particular, his *Game Theoretic Analysis of Voting in Committees* [Cambridge: Cambridge University Press, 1984]). But even in the absence of a complete specification of the game, Schwartz's analysis is rich in insight and offers real potential for a deeper understanding of the collective-choice process.

Finally, a word about style: readers familiar with Schwartz's previous contributions to the literature will be familiar with his penchant for acronyms. With an entire book on social choice, Thomas Schwartz goes wild. So while I found the substance of this book to be informative and important, I confess to becoming exasperated by the author's continual invocation of the principle of curious terminology and symbols (CUTESY).

DAVID AUSTEN-SMITH

*University of Rochester*

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**Uneven Development: Nature, Capital, and the Production of Space.** By Neil Smith (Oxford: Basil Blackwell, 1984. xv, 198 p. $34.95, cloth; $11.95, paper).

This book combines two purposes. The first, represented in chapters 1-3, is to "renovat[e] the terribly archaic conception of nature that dominates western thought" (p. vii). The second, carried out in chapters 4 and 5, is to set forth a theory of uneven development under capitalism.

Smith opens with a critique of the bifurcated "bourgeois" ideology of nature. In this vision, nature is simultaneously *external* to man and *universal*, encompassing man and human society. This bifurcation is ideologically convenient, if contradictory, because *external nature* is a fit object for conquest and domination. At the same time, *universal nature*, as a source of virtue, or at least inevitability, can be appealed to as a justification for any current arrangement.

Smith develops by contrast his interpretation of Karl Marx's views on the man-nature relationship, building upon and criticizing previous work by Alfred Schmidt. He takes a dialectical approach: nature is perhaps external to man, but there is a "metabolic interaction" between the two. "Nature becomes dialectical by producing men as transforming, consciously acting Subjects confronting nature itself as forces of nature" (p. 20). Nature, in other words, is changed by man; "natural things gain a new social quality as use-values" (p. 19). Smith extends this formulation with his concept of the production of nature: the "natural landscape" is "more and more the product of social production" (p. 32). Indeed, original or "first" nature is so comprehensively reconstructed by the logic of exchange values under capitalism that the result is something new, "second nature."

Smith's argument here is interesting, and I hope it will stimulate more concern with values and symbols as a counterpoint to the common focus on material aspects of urbanization. Unfortunately, in his statement of a Marxist position, Smith seems to have lost the insight that scientific and cultural concepts of nature are inherently ideological. His own sparring with Schmidt, whom he identifies explicitly as a representative of the Frankfurt school, illustrates that not only bourgeois but even Marxian concepts have an ideological character. Also I suspect that most readers, like me, will not discover what difference Smith's concept of man and nature makes for his theory of uneven development. In the preface he denounces the notion of nature's externality, the idea of space as "a basic coordinate of reality, a field . . . within which material events occur" (p. ix). From this perspective, he says, "human activity does not restructure space; it simply rearranges objects in space." And so, finally, "the symptoms of spatial restructuring appear as just so many separate processes at separate scales with very separate causes and explanations." But Smith himself analyzes uneven development as a process of spatial differentiation and equalization and, more precisely, as the movements of capital in space. However much capital mobility is linked to the specific political economic forms of capitalism, it is still a rearrangement of machinery and roads, housing and schools, in the territory. Certainly, Smith adopts a special vocabulary for his theory, a vocabulary in which the concept of social production of space is prominent. The need for this vocabulary is asserted but not argued. I conclude that reconceptualizing nature is a fascinating but secondary theme in this book, an original theme with unrealized promise.

In his theory of uneven development, Smith has borrowed freely from David Harvey's interpretations of the relations between capitalism and the built environment. The concentration and centralization of capital also concentrates labor, resulting in a spatially differentiated landscape. Capital's continual search for relative locational advantage, especially for low-wage labor—what can be called the "universalization of capital"—creates a countertendency toward spatial equalization. Crises in capitalism, global or sectoral, are unevenly distributed in space, giving rise to the various "urban" and "regional" crises which are much in evidence today. Very briefly, "the dialectic of geographic differentiation and equalization is ultimately responsible for the pattern of uneven development" (p. 131).

This analysis is essentially economic, but Smith does bring politics in with his discussion of the different processes creating spatial inequality at the urban, national, and international levels. State action at the national level, he believes, obstructs the international leveling anticipated by Marx, because the nation-state must protect "national capital." I am dissatisfied with this sparse treatment of the political process. Granted that nation-states influence the patterning of development, cannot and do not state agencies at the local level, and international coalitions and cartels, also affect development? Can states be so directly identified with *national* capital in a multinational era? Aren't there other influences on state action not so easily reducible to protecting national capital, such as the interests of local capital or labor? And don't these constellations vary widely from state to state? I am concerned that Smith's political theory is undeveloped and perhaps purposely neglected.

And from another side, Smith simply asserts that a tendency for a falling rate of profit is endemic to capitalism and that seeking new markets and sites for investment—exerting an equalizing pressure—is the natural response. He does not confront the claim from neoclassical economics that it is spatial differentiation which is natural to the economy, differentiation that promotes system efficiency. This is especially curious because he wholeheartedly endorses the "ground-rent system" as an efficient allocator of urban space within cities. Further, Smith ignores the possibility of alternative ways for capital to seek relative locational advantages from their current location by manipulation of the political and economic environment.

Smith has bitten off more than a mouthful, combining into one book what might as well have been two separate essays. There are flaws here, but I think we must pay attention to the problems that Smith formulates (1) on the character of urban ideologies and (2) on the interconnections of uneven development across spatial scales.

JOHN R. LOGAN

*State University of New York, Albany*

**Mechanics of the Middle Class: Work and Politics among American Engineers.** By Robert Zussman (Berkeley: University of California Press, 1985. viii, 269 p. $27.50).

This often interesting book makes a case for thinking of higher technical workers as divided in their social allegiances—identifying with businesspeople and top managers at work, but with the broad "working middle class" in politics. The book, which is based in a study of 80 engineers working at two New England companies, is an admirable ethnographic study of the work of engineers, but its political conclusions are highly debatable. Consequently, it may prove more valuable to sociologists interested in work and occupations than to political scientists interested in the politics of professional workers.

The main political point is this: although engineers are closely allied with busines people and managers at work, they are not closely tied to business interests in political life. Instead, engineers see themselves as part of a self-sufficient middle class distinct from both the dependent poor and the privileged rich. Together with others in this "working middle class," their concept of government is negative and cynical. They see it as overblown, and they see themselves as having little stake in its services. "In the engineers view," Zussman writes, the benefits of a welfare system "have been extended to those (the rich and the poor) who do not meet the obligations of citizenship as taxpayers" (p. 214).

It seems likely that Zussman is making too much of a particular historical situation. His field work was conducted during the peak of "tax-revolt" sentiment in the urban Northeast. Engineers have perhaps always been predominantly conservative in this country, but other strata in Zussman's "working middle class" have not. Even among engineers, the views expressed here probably reflect, to a considerable degree, the fiscal problems of government during the period, together with the weakness of the divided Democrats and the effective mobilization of "antigovernment forces."

Some of Zussman's other conclusions also can be questioned. His conception of where engineers fit in the U.S. class structure is too much based on the engineers' own sentiments, and his rejection of the term "professional" to describe the engineers' work is not based on a careful analysis of professionalism.

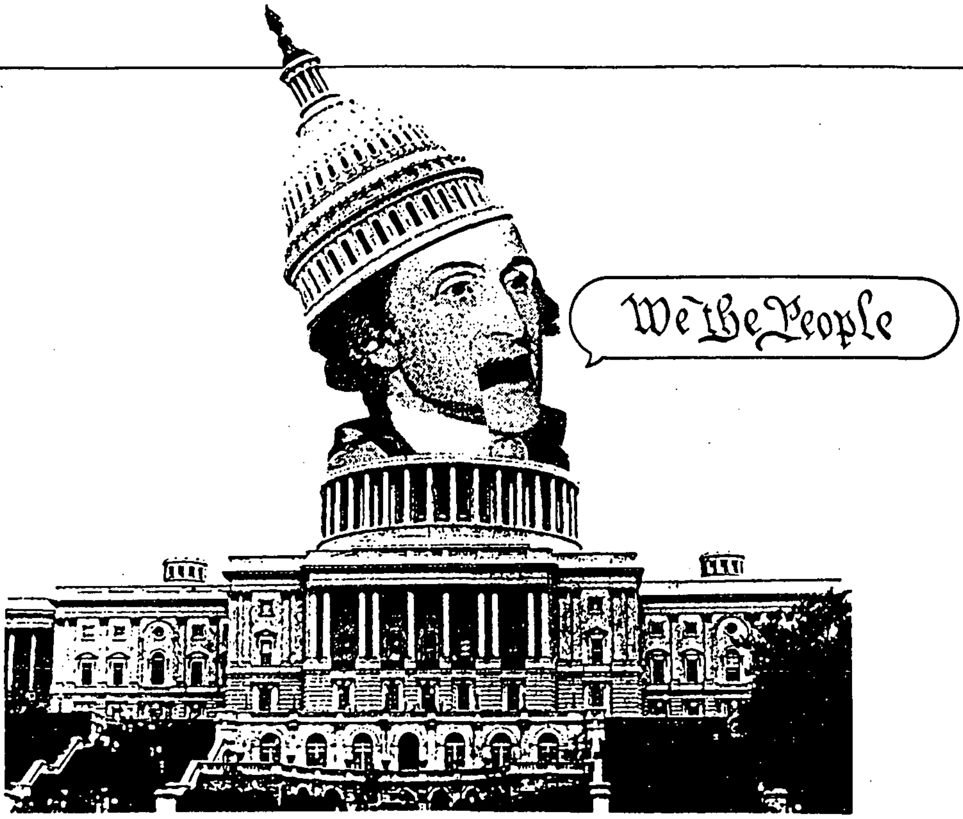
But what a fine study of engineering work Zussman provides! He shows how engineers' daily movements, market power, and built-up skills limit managerial control over their work. He also shows how engineers are closely tied to managers through common responsibilities, career movements, and shared assumptions about cost and profitability. He shows that engineers who have come up from the ranks are the only group to develop a true commitment to engineering work rather than to managerial careers. He provides a useful understanding of why neither unions nor professional associations have made significant inroads among engineers. Throughout he demonstrates a deftness in middle-range sociological analysis.

Harold Wilensky once called higher-level technical workers "hybrid men"—professionals in their training and skills and managers in their willingness to fit into bureaucratic roles and to accept cost and profit criteria. Zussman's work should lay to rest some notions about technical workers that have developed since Wilensky's time. Against the brave new scientific world of the postindustrialists, he shows that most engineering work involves a few basic math and lab skills and is largely learned through practical experience. Theoretical knowledge is important for only a few. He also challenges the postindustrialists' view that the new high-tech, science-based industries produce "new men." Instead, engineers in the "advanced" industry he studied were little different from engineers in established industry. Against all Thorstein Veblen-inspired ideas about an independent technocracy, Zussman shows that a great majority of engineers see no conflict whatever between the logic of efficiency and the logic of profit. Cost and profit are simply accepted as criteria of technical efficiency. Against Marxist notions that technical workers are following in the footsteps of the old craft workers toward "proletarianization," he finds sturdy bases for continued privileges and autonomy in the market power and skill levels of engineering work.

By dispelling these influential but wrong ideas, Zussman helps bring us closer to an appreciation of the true situation of these well-integrated men in the middle who are both managers and skilled workers, practically educated, but with significant opportunities for advancement.

STEVEN G. BRINT

*Yale University*



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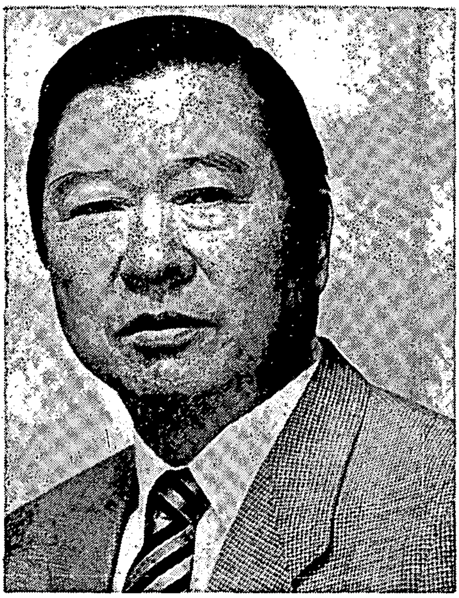

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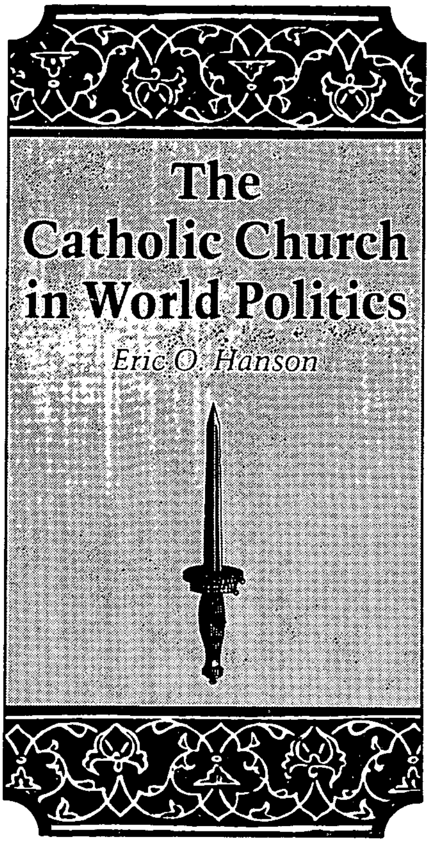

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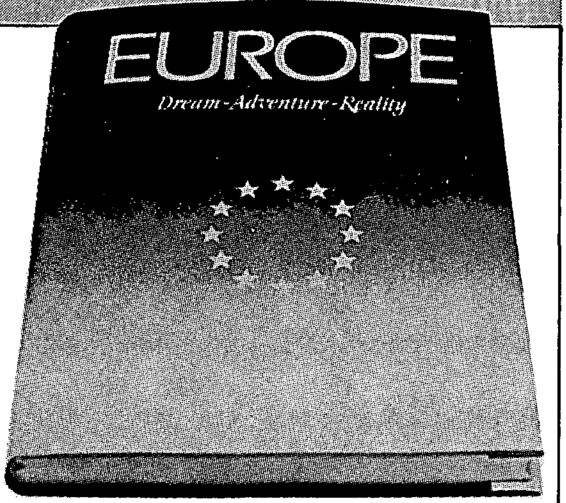

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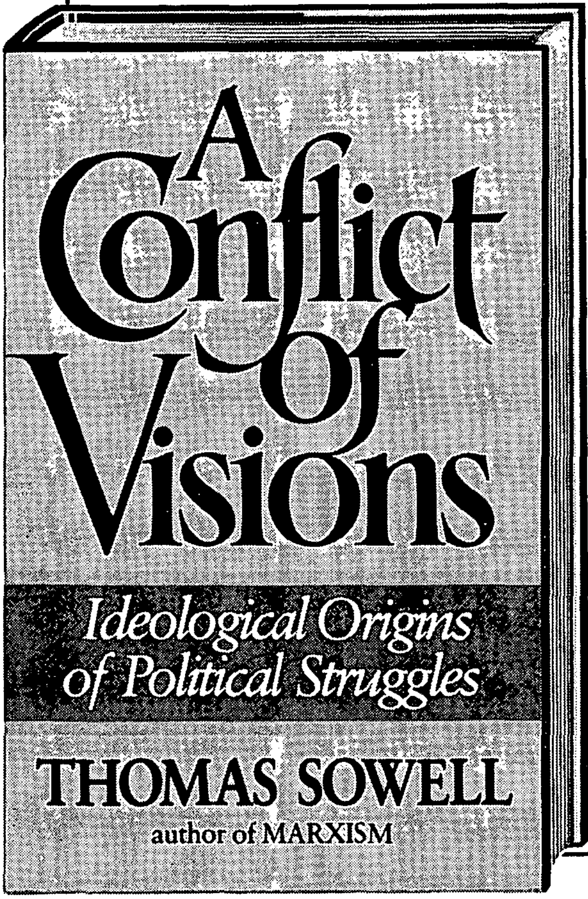

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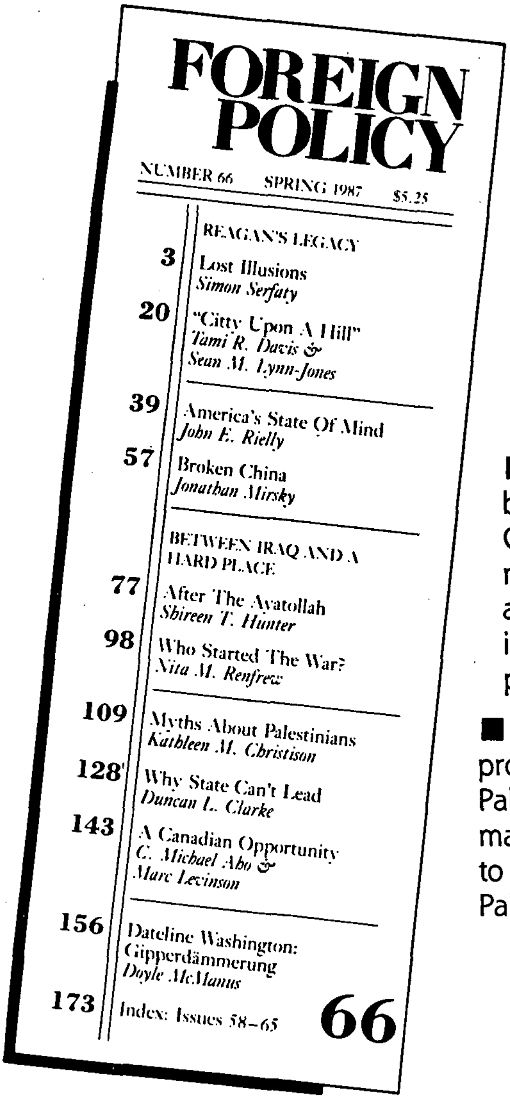

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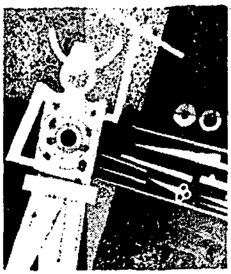

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